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DISCUSSION ON THE KENTUCKY AND INDIANA BRIDGE.*

J. W. SCHAUB, M. Am. Soc. C. E.—The paper of Mr. Moulton on the Kentucky and Indiana Bridge is a valuable record as to the results of tests, etc., and it is to be hoped that Mr. Moulton will supplement his paper so as to discuss some of the salient features of this remarkable engineering structure. The novel and intricate systems of cantilevers are the work of the late Mr. C. Shaler Smith, to whom Mr. Moulton gives just tribute. A similar arrangement of cantilever spans are now being built by the Union Bridge Company at Poughkeepsie, N. Y.

Mr. Moulton says: "No unusual problems presented themselves in the design excepting those natural to the system of web members, and the series of cantilever spans continued to such an unusual extent."

As I am somewhat familiar with the methods of computation employed by Mr. Hemberle, and the details used in the construction of this work, I was naturally disappointed in seeing Mr. Moulton omit a discussion of some very important features. The changes made in Mr. Smith's original plan, as to the style of trusses, depth, etc., possess many advantages as to economy of design; and this, in connection with the use of the Launhardt formula, as employed here, permitting much higher unit strains, made a vast difference as to the final cost of the superstructure. However, it will not be out of place to discuss some of the marked differences in the two designs. Mr. Smith's plan, which provided for a floor riveted between vertical posts, possesses many advantages over a floor suspended from the lower chord. It is only necessary to examine the two kinds of floors when in use to be convinced of this fact. However, in the Kentucky and Indiana Bridge the floor beams are screwed up against the lower chord, making a rigid floor if properly screwed up.

* Transactions, Vol XVII, page 111 (September, 1887). The Kentucky and Indiana Bridge, by MACE MOULTON, M. Am. Soc. C. E.

The use of a tight fitting pin at the intersection of the web members in the trusses was here used ostensibly for the purpose of shortening the compression members by one-half their total length, and considering them as fixed-ended where they are held by the diagonals at their intersection. To be sure this is correct as far as our principles of statics go; but it is easy to see that distortions must take place in the web members themselves, as soon as a load comes on a structure framed in this manner. For example, let us suppose the trusses to be framed with a camber when there is no strain in any member. This is equivalent to laying the trusses flat on the ground. The web members now intersect at some point below where they would intersect if the trusses had no camber. Now, when the load is put on the truss, the chords assume their normal position. The point of intersection must rise until, finally, when the entire camber is taken out of the trusses the members will have assumed their normal positions, provided the web members are not fixed to each other at their point of intersection. Where they are held as in the Kentucky and Indiana Bridge they will only tend to assume their normal positions and consequently distortions must take place. I would like to know from Mr. Moulton, what is the maximum difference in the length of the upper and lower sections of the diagonals in the 360 feet span? Knowing this difference it is easy to calculate the theoretical movement of the intersection of the diagonals during the transition from camber to no camber in the trusses. This difference would represent the total movement that would take place if the intersection were not fixed and the eye-bars had no sag.

Furthermore, a member held at any point is not necessarily fixed-ended at that point unless it is in some way prevented from assuming a line of double curvature under compression, thus:

Is it fair to make a test on a column with one pin end and one square end in a testing machine, and use the results as a precedent in designing a structure, wherein the conditions are so much at variance with those of the testing machine? Aside from the elements such as imperfections in workmanships, vibration and impact, do we approach anywhere near the same conditions as those of the testing machine. First, the members are not shortened by one-half their total length; second, they are not fixed-ended; and last, they are subject to distortions imposed on them by passing loads. The specifications say that the numerator in the column formula (Rankine's) shall be determined by the Launhardt formula, but Mr. Hemberle wisely disarmed criticism in using the largest sectional areas for compression members, obtained either by substituting directly 13 000 in the numerator of the column formula, or by taking the permissible working stresses as determined by the Launhardt formula. However, do the results of the tests on the full sized columns, especially the post made from compres-

sion steel, justify the use of 13 000 pounds in the numerator of the column formula?

Another prominent feature here, which is also a characteristic of several very important structures recently built, is the use of eye-bars in unison with built members to resist alternate tensile and compression strains. In the top chord of the 360 foot span will be found three panels at each end of the span where eye-bars are placed outside of the built chord sections. As the tensile strains here are large and the compression small, it was thought advisable to avoid excessively heavy built members and consequently heavy field riveting at the splices, where eye-bars could be used to advantage; and I believe it was here specified that the eye-bars and corresponding chord sections should be assembled in the shop, to avoid any discrepancies in their lengths. The use of eye-bars and built members in unison to resist tensile strains is very doubtful, especially where it is extended over a great many panels. The built member, owing to its continuity, gets more than its share of the strain in every case I have seen. This is partly owing to the play the eye-bars have on the pins, the bending of the pins themselves, and also to the strains which take place in the heads of the bars, which do not occur in the built member. In a very important structure recently built, this fact is exemplified to its fullest extent in the excessive deflections obtained, which are accountable in no other way. In the Kentucky and Indiana Bridge this combination of eye-bars and built members was used so moderately that it is hardly questionable as to its effects, and could not have been avoided without very much increased cost to the contractors.

The increased unit strains which were allowed by the Launhardt formula is another striking feature. The specifications say: "The maximum and minimum stresses in compression and tension, as found by the before-mentioned loads, with a factor of safety of not less than five, are to be used in determining the permissible working stress in each piece of the structure, according to Launhardt's formula, as follows:

$$a = u\left(1 + \frac{\text{minimum stress in member}}{2 \times \text{maximum stress in member}}\right)$$

in which

Tension is algebraically —
Compression " " +

a = permissible stress per square inch.

For double refined iron in tension (links or rods):

u = ... 9 000 pounds per square inch.

For double refined steel in tension:

u = ...14 000 " " "

For rolled iron in tension (plates or shapes):

u = ... 8 500 pounds " "

For rolled steel in tension:

$u =$ .....14 000 pounds per square inch.

For rolled iron in compression:

$u =$ ..... 7 500 " " "

For rolled steel in compression:

$u =$ .....13 000 " " "

The Launhardt formula, as before given, embodies the two formulas given by Weyrauch, inasmuch as the formula becomes, when the minimum and maximum stresses have the same sign + or —.

$$a = u\left(1 + \frac{\text{minimum } B}{2 \times \text{maximum } B}\right)$$

when they have opposite signs

$$a = u\left(1 - \frac{\text{maximum } B^1}{2 \times \text{maximum } B}\right)$$

in which

Max. B = the greatest stress upon the member, whether tension or compression.

Min. B = the least stress of the same kind.

Max. B^1 = the greatest stress in the opposite sense.

See paper of Mr. Joseph M. Wilson, M. Am. Soc. C. E., page 391, Vol. XV. However, Weyrauch says that u = ultimate strength per unit of section for any number of repetitions of load. In the Kentucky and Indiana Bridge u = for rolled steel in tension 14 000 $\times$ 5 (factor of safety) = 70 000 pounds per square inch, which should be the ultimate strengh for any number of repetitions of load. What do the tests on full-sized eye-bars show? The ultimate strength of 12 bars tested to destruction varied from 63 500 to 78 500 pounds per square inch.

I presume the bars were tested in the usual manner, which is practically a single application of the load. Judging from the results of the tests, were the values of u here properly taken?

In the 267-foot skew cantilever span we find in one case a working stress of 17 500 pounds per square inch in tension. Granting the ultimate strength of large bars at 70 000 pounds, we obtain a factor of safety of four for dead load and live load combined. Here the ratio of dead load to live load strain = 1. Therefore, if 5 be the factor of safety for live load, the factor of safety for dead load must be 3. Or if 6 be the factor for live load, the factor for dead load must be 2, and so on.

The aim of the Launhardt formula is certainly very scientific, but would it not be simpler to fix upon a factor of safety for dead load, and a factor of safety for live load, obtain separately the sections required for each, and add them together for a final result? This would be very simple for members subject to only one kind of strain. For members subject to alternate strains we could use a certain percentage of the lesser strain and add it to the greater for the resultant live load strain. As

the Launhardt formula requires the maximum and minimum strains to be determined for each member, it would seem that in an ordinary discontinuous span the dead load strains would be the minimum. However, it is easy to see that for the web members the counter-strains would tend to reduce the dead load strains, as the live load is backing off the bridge. This necessitates finding the counter-strains for all web members and combining them with the dead load strains to obtain the minimum strains for web members. Does a refinement like this seem rational? Would it not be simpler to say, for example, the factor of safety for live load shall be 6 and the factor for dead load shall be 3, and the sections for each shall be added for a final result?

The specifications required steel for compression at 80 000 pounds ultimate, and steel for tension at 70 000 pounds ultimate. Would it not be advisable to use the same material for both tension and compression, especially where members are subject to alternate strains. In the Lachine bridge, recently completed, no distinction was made, and a uniform steel at 60 000 pounds ultimate was used throughout.

The tests made by Mr. Moulton as to the effects of shearing and hammering steel plates are very interesting, and tend to establish a valuable precedent. The specifications required all holes to be punched and then reamed one-eighth inch larger, leaving the holes of the proper diameter as required by the size of the rivet. As the ultimate strength for steel is gradually being reduced, it is to be hoped that practice will eventually establish a uniform steel at 60 000 pounds ultimate, and avoid the expensive processes of planing the edges of sheared plates and reaming holes for rivets.

PALMER C. RICKETTS, M. Am. Soc. C. E.—The difference between the allowed shearing resistance of steel and iron in the specifications under which this bridge was built, 10 000 and 7 000 pounds per square inch, seems to be too great. This is here referred to because it is believed that the difference usually allowed in favor of steel, about 33 per cent., is greater than the conditions existing in steel and iron structures and the strength of the material warrant.

Although the Watertown experiments seem to show generally that this amount may be allowed, most of them were made with single riveted quarter-inch plates, with smaller pitch and rivets than is generally permissible in bridge practice; and if the difference in resistance of either metal in punched and drilled joints be considered, the few directly comparable experiments on double riveted joints with thicker plates made there reduces this percentage appreciably.

The results of English experimenters bear out this inference and would seem to indicate that if 7 500 pounds per square inch be taken for iron rivets in shear, the amount allowed for steel should be more nearly 9 000 than 10 000 pounds.

As far as failure by nearly pure shear is concerned, experiments of the

writer, published in the Transactions of this Society for March, 1887, on iron and steel, three quarter inch round rivet material, some of the latter taken from lots used for rivets in the bridge which is the subject of this paper, show that the resistance of steel of 0.10 per cent. to 0.14 per cent. carbon and about 0.40 per cent. manganese, with 58 000 to 64 000 pounds ultimate tensile intensity, is not more than 15 per cent. greater than that of iron. The considerable difference in resistance when the cutting edges are sharp and rounded, is also shown there.

It is interesting to notice that if in the results of the tests of the two ong columns given on pages 160 and 161 we disregard the strain for the stress on the column below 20 000 pounds, regarding this amount as necessary to begin a somewhat uniform strain in the central measured length which the first compressive increments in the tables show to be the case, we get, by using the mean of the remaining strain increments up to 14 000 pounds per square inch, beyond which the column would not be liable to be stressed, co-efficients of elasticity of 27 200 000 and 28 600 000, and for stresses up to 30 000 pounds per square inch 26 500 000 and 27 400 000.

The mean compressive co-efficient for the eight built steel posts given by Mr. J. G. Dagron, M. Am. Soc. C. E., in his paper read at the Annual Convention in July, 1887, had an average value of 27 600 000, varying from 25 800 000 to 29 500 000.

In these experiments it is seen that the value of this quantity for these built pieces of structural steel, stressed as they would be in place, is not very much less than its average value, about 29 000 000, determined by experiments on small pieces.

A consideration of the tests of the eye-bars given in this paper, compared with those of the three-quarter inch rounds from the test ingots, together with a comparison of other results of the same kind from some Watertown experiments, shows that the difference in elastic limit in favor of the metal from the test ingot varies in these cases from 10 to 33 per cent., and in ultimate resistance from 0 to about 15 per cent., in one case, reaching 26 per cent. The elastic co-efficients seem to remain practically the same.

Samuel Tobias Wagner, M. Am. Soc. C. E.—Among the extremely interesting and well-presented features of Mr. Moulton's paper is the record of tests where comparison is made between those made on a ¾-inch round bar and those cut from the section as finally rolled, and again is this comparison more beautifully shown where the tests are given on a ¾-inch round on a specimen cut from the bar, and on a full-sized manufactured eye-bar. If in steel a closely approximate relation can be established between tests prepared on ¾-inch round bars and several sizes of rolled shapes, plates and bars, all from the same heat, so that upon the ¾-inch round tests the heat could be at once accepted or rejected; the time saved in the completion of such a contract as Mr. Moulton's paper de-

scribes, would be considerable, and the satisfaction to both inspectors and manufacturers would be of great value.

Some four years ago this point was presented very forcibly to the writer at a prominent steel works in Pennsylvania, while he was acting as inspector under some rather poorly prepared specifications, where, for want of some sufficiently prepared and discussed matter on this subject, the test pieces to determine the acceptance of the heats were prepared from plates rolled to certain widths and thicknesses in order to give on them approximately the same amount of work as the final section would have, and no tests were made on a ¾-inch round, although the steel makers claimed that the results obtained from these tests would be satisfactory. The material was a 60 000-pound steel, with a required elongation of 23 per cent. in 8 inches, and had to be accepted from the steel mills in the shape of blooms, ranging in size from 3 by 3 to 7 by 10 inches, and which were to be rolled down at another mill into sizes varying from 1½ by 1½-inch L to 9-inch deck beams. Upon these the final tests were to be made under a very rigid inspection. It became necessary therefore to have several different sizes of rolled specimens from which to cut tests in order even to approximate the amount of work and reduction from the ingot.

From a careful examination of Mr. Moulton's results on ten different heats, I have selected some eighteen different characteristic tests, and striking a rough average from these, I find:

First.—The elastic limit is higher in the ¾-inch round than the other tests, by about 2 100 pounds in thirteen tests, while it falls to about 1 100 pounds below in five others.

Second.—The ultimate strength shows in fourteen tests a greater strength in the plate specimens than in the ¾-inch round, averaging 2 300 pounds, while in four tests the ¾-inch round shows a greater average strength of 3 100 pounds.

Third.—The elongation is decidedly the least fluctuating, and always shows greater in the ¾-inch round than in the plate test, but the average in the eighteen tests is only about 2.4 per cent. higher.

Mr. Moulton's results are extremely interesting and valuable, and agree with what experience I have had and results I have seen on the subject, although his are by far the most complete. It is to be hoped that some one else will give us more data on this subject, so that these results may be confirmed, although, I believe, for steels from say 60 000 to 85 000 pounds per square inch ultimate strength, the fluctuations will coincide very closely with the results shown here. Mr. Moulton remarks that the specifications called for the tests to be made on a ¾-inch round, and this is an encouragement for its future use, where the check tests are as good as these are.

The results obtained from the 7 by 7 inch square ingots, I think, are unusually good for such a severe method of testing the quality of the

material, although I am glad to see they were abandoned, as certainly the danger of unjust results from them is very great.

The matter of cast-steel clevises is one of great interest to bridge builders, as it is a step toward the substitution of steel castings for what are now expensive forgings. The results obtained are not surprising, as there seems to be very great difficulties attendant upon the manufacture of small steel castings, but it is to be hoped that the steel makers will soon solve the problem. It would be of interest to know under how great a head of metal these clevises were poured, and in what position they were moulded.

If in the discussion Mr. Moulton could tell us something about the method of working the steel in the shops, that is, whether it was drilled or punched and reamed, or simply punched, and also whether special caution was taken to avoid sharp corners and edges, it would add a few more interesting items to his already complete description.

JOSEPH M. WILSON, M. Am. Soc. C. E.—I should say that the moving train load assumed for this bridge, in view of modern service requirements, or even those of ten or more years ago, was too little. After considering carefully the lengths of spans, I should not have made it less than 3 000 pounds per foot, and perhaps more.

The formula adopted for the maximum and minimum stresses in compression and tension appears to be for stresses only of one kind (all compression or all tension). No modification is made for stresses acting in opposite directions, although from the type of the structure I should judge that such stresses did occur. I hardly understand why the paper should make use of the term "factor of safety of not less than five" in connection with the use of this formula. The stresses introduced into the formula for the various cases must evidently be the actual stresses, not these stresses divided by five, and the value of u is assumed at various quantities for different materials so as to give the permissible stress a per square inch. While the process of calculation may have been correctly followed out, the explanation of the use of the formula is certainly not clear. Also, on page 136, there appears to be a misprint. It should be:

a = permissible stress per square inch.

u = for double refined iron in tension (links or rods) 9 000, etc.

u is here, as I take it, an assumed permissible stress per unit of section for any number of repetitions of load, adopted from experience or precedent, and not by any means one-fifth of a supposed ultimate strength for the material in question.

The values of u given in this paper for iron are the same as I used some years ago with the same formula, although I also used Weyrauch's second formula for pieces subjected to stresses acting in opposite directions. These values were printed in specifications of mine, issued for private use only in connection with the Pennsylvania Railroad. The

formulas and values of u were afterwards modified to those given in my published specifications (see Transactions Am. Soc. C. E., Vol. XV, June, 1886), as presenting in my view an improved practice.

The formulas for reduction of members in compression vary slightly from those which have been used by myself. The question of requiring the lines of the neutral axis of all pieces meeting at a joint to meet in the same point, or the placing of pins where used, in the neutral axis, and if not so arranged, the necessity of considering the bending stresses so produced, seems to be ignored.

In the provision for wind strains and vibration, no computation seems to be made for the case of a moving train. The results with a moving train will be different from those of a static load for the whole length of the span.

The method of computing the shearing stress on girders with stiffeners differs from my own theory on the subject (see Transactions Am. Soc. C. E., Vol. XV, June, 1886).

The requirements of the specification for iron and timber and for workmanship are very meagre. The mere statement that they should be "as usual in first-class work" would seem to me hardly sufficient in a "first-class" specification.

I regret that Mr. Moulton has not given us a summary of what practical inferences he would gather from the results of his tests as shown in his tables. Of course any one can by an examination of these tables draw inferences, but I would rather have seen his own conclusions.

On page 163 the drawing of clevis appears to be inaccurate. The connections of the arms with the nut are evidently not correct.

Concerning the question of adopting tests from the ingot or the finished material, for the acceptance of the latter, it would appear from the results on pages 165–6 that ingot tests give higher results than finished material tests. If the former are adopted, as seems to be urged upon engineers by manufacturers, an allowance should be made for subsequent deterioration, which might be some established percentage.

C. L. Strobel, M. Am. Soc. C. E.—A record of the construction of a large bridge in such detail as presented by Mr. Moulton is very interesting and valuable, and he merits our thanks for his labor, which has been very considerable.

Mr. Moulton's experience with cast-steel agrees with my own. In the present stage of its manufacture, and in all probability for a long time to come, this material is not suitable for use in bridges when subjected to tension, because its strength is too uncertain. I have, however, used it for such parts as bolsters, shoes and pier-boxes, to great advantage. These parts are mainly in compression, massive in size, and the stresses are low, so that there is ample margin for safety even if honeycombing or the other defects to which this material is subject exist. Cast-steel has the great advantage over cast-iron, that, if a suit-

able grade is selected, and it is properly annealed, it is tough and malleable. It does not have the brittleness of cast-iron, and it has the advantage over wrought-iron, that it can be cast into massive and complicated forms, which it would be impossible to make by riveting together plates and angles and obtain equal efficiency.

The results obtained with sheared and planed steel plates indicate simply less elongation for specimens having one edge sheared and the other planed, as compared with specimens having both edges planed, the ultimate strength remaining about the same. A marked difference would have been found, however, in the ultimate strength and other qualities of the specimens had they been subjected to vibratory action while under stress. A slight crack, rent, or a sharp indenture on the edge of a bar or plate is a point of weakness in steel greater than in wrought-iron (provided the direction of the break in the latter case is across the fiber) for the reason that steel is a homogeneous material and readily yields to new conditions, so that incipient fracture acts like an entering wedge, and failure gradually takes place by an accretion of small increments of fracture. One of the striking features of the behavior of steel columns when tested to their ultimate strength is their sudden collapse soon after the elastic limit has been exceeded. The metal bulges between rivets or other point of support, flowing away from the line of stress suddenly and freely, therein acting very differently from the behavior of wrought-iron under similar conditions. The latter retains its form longer after the elastic limit has been exceeded, and is more apt to fail by bending as a whole; or, if it gives way by bulging or buckling locally, it does not do so as freely nor as suddenly as steel.

I should like to ask Mr. Moulton what object there was in using pine for the upper layer of plank in the wagon way, and oak for the lower layer placed diagonally? The reverse is the usual practice; the lower layer is made of pine placed at right angles to the joists so as to make the span between them as small as possible, and the upper layer is made of oak so as to have a hard material as the wearing surface, and the plank in this layer is placed diagonally or lengthwise of the bridge. I should also like to ask whether the floor-beams are without connection to the lower chords, as drawn on Plate XIX, in which case it is difficult to see that there is any chord to the lateral system.

In both the designs of superstructure as adopted and as originally proposed, double systems of triangulation are used, which give rise to a certain amount of ambiguity in the stresses not desirable though not seriously objectionable. How much of the weight of the suspended span in the cantilever system is carried by one and how much by the other of the two systems of triangulation, is dependent upon their relative deflection and the accuracy of length and fit of their parts. The design adopted is certainly more economical of material than the other, and it has the further advantage that the chord lines are straight be-

tween the apexes of each system (not broken as in the top chord of the other design), whereby one of the elements of abiguity existing in the latter is eliminated.

HENRY B. SEAMAN, M. Am. Soc. C. E.—If it is at all feasible to use steel rods with screw ends, the precaution of a soft steel, with fillets at the bottom of the threads, would seem to make it so; but even then a roughly cut thread is unavoidable occasionally, and is an element of weakness.

The real test for the rods will be in the bridge, where they are subjected to a repetition of strain and to vibration, rather than in a testing machine, where the strain is continuous; as vibratory strains will more effectually develop cracks.

The fact that the upset ends are of greater diameter than the body of the bar, tends to relieve the screws from the effect of impact and thus to prolong the life of the bar; but experience has shown that with ordinary threads, this relief is not always sufficient to save the rods, as they have broken in the thread, showing in the fracture rings of rust, and indicating that the bar was broken in increments.

Mr. Moulton's tests of annealed specimens show in every case a deterioration by annealing. The elongation is reduced, while the reduction of area at the point of fracture is increased, indicating a more plastic but less tenacious material. This is probably more marked in a small test specimen than it would be in a finished bar, but would also occur in the latter to some degree. It is probably due to decarbonization, either by being kept too long at the refining heat, or being cooled too slowly, possibly in the air. When we consider the great influence heat has upon the quality of steel, it would appear advisable to anneal with every lot of bars a specimen of the same material, of full section of the bars, and about 18 inches long, which may be afterwards tested for over annealing; the test is inexpensive and easily made.

There is a clause in the specifications for superstructure which provides that "rivets will not be used where they may be subjected to tensile strain." This is not an uncommon specification, but if it were strictly enforced, it would throw out the connection for lateral bracing shown on Plate XIX. Rivets are preferable in shear on account of the additional resistance of friction; but where tension is necessary, as is shown here, I think it is better to specify some limiting tensile strain.

Rivets are made of the best quality of material, and when shop-driven, the shank of the rivet undergoes less "punishment" than an ordinary upset rod. If sufficiently strained, rivets will work loose under shear, as well as under tension; and if rivet heads of hard steel drop off, it would seem to be the fault of the material rather than the kind of strain, for they drop off under either strain.

I do not wish to be understood as advocating tension on rivets. It should be avoided wherever possible. But I do not think it such a

"bugbear," as is often considered. I think the tensile value of a rivet should be considered a function of the shearing value.

C. E. EMERY, M. Am. Soc. C. E.—The ultimate tensile strength of steel boiler plates for the United States Navy and the better class of marine works has for a number of years been limited to 60 000 pounds; a latitude of from one to one and one-half thousand pounds above and below that figure having however been allowed to meet practical conditions at the mills. Substantially these limits have been fixed also for the steel used in the new naval cruisers. Even with steel as low as this, initial strains are introduced by punching and shearing which materially reduce the strength of the plates, though the difficulty may be practically overcome by reaming the holes and planing the edges.

It is believed that a reaction will soon set in in regard to increasing so largely the sections of members subject to reverse strains. The question is very materially modified by the periods of rest available between the applications of strains. In the experiments on which the original formulas were founded, the reverse strains necessarily followed each other in comparatively rapid succession in order to get results in a reasonable time, and moreover the applications were kept up continuously until failure resulted or particular experiments were abandoned. Had there been periods of rest after the application of reverse strains for a time, the molecules would have re-established their equilibrium and the material been as good as before. My recollection is that in the experiments it was necessary to carry the strains nearly up to the elastic limit of the material to produce rupture under conditions of continuous application. Bridge members cannot receive such severe tests when the reverse strains are well within the elastic limit, the periods of rest between maximum loads allowing the strength to become restored without fail. This is proved by the life of iron and steel rails, which even on a bad foundation remain intact under reverse strains very much greater than ever occur in any other form of construction.

Mr. THEODORE COOPER.—Allow me to ask, did I understand you to say that your experience was to ream the holes in steel?

Mr. EMERY.—In the construction of boilers we permit the builders to punch the holes in steel of 60 000 pounds tensile strength and afterwards ream them.

Mr. COOPER.—Do you recommend it?

Mr. EMERY.—I do not recommend it. I would rather see the holes drilled. I can only say that it seems a necessity to permit it, as plenty of boilers are made from steel in the same way as iron without even reaming the holes. The method of reaming seems to answer under practical conditions and therefore under the circumstances is allowed. Steel is, however, so much abused in boilers that I sympathize strongly with those who will not permit its use at all.

THEODORE COOPER, M. Am. Soc. C. E.—I am rather pleased with Mr. Schaub's remarks in regard to the Launhardt formula, but he don't go far enough. What are Launhardt and other similar formulas based upon? In all the discussions which have recently taken place in regard to the use of this form of formula I have found no evidence that the writers have studied the original experiments from which this form of formula has been derived. The discussion has not been upon the merit of the form but rather upon the proper constant 1, 2 or 3 to be inserted before the ratio of the minimum and maximum stresses.

The very interesting experiments made by Wohler in 1867, and first published in English in *Engineering* of 1871, are the basis upon which these formulas are built.

Wohler extended his tests over several years, and submitted some of his test pieces to as high as 132 000 000 applications of the strain. He made some of his tests by direct tension, but the largest number were made by tensional and bending tests very ingeniously arranged so as to confine the tests within limited ranges. While he formulates his results in strain per unit of area, any one must recognize that these fiber strains would, under any method of calculation, be very doubtful in their amount. After years of investigation he was, like every true experimenter, very cautious in drawing any deductions. He does state that "variations of strain may with equal security take place within the following limits, it being of course assumed that in all cases the maximum strain is less than that required to produce fracture under a static load."

For iron subjected to tension and compression.

— 17.120 pounds	to	+ 17.120 pounds
0 "		+ 35.310 "
+ 25.680 "		+ 47.080 "

etc. for other cases.

The discovery of this apparent anomalous action of a metal which had an elastic limit, as result determined, of 35 000 pounds, breaking when subjected to strains not exceeding one-half of this amount, led to the absurd and unscientific term "fatigue of metals." This result was totally at variance with the acceptance of the perfect elasticity of the metals as heretofore accepted.

A careful study of the original experiments led me at that time to the belief that the true explanation of this so-called fatigue of metals was in the idea that this iron did have an elastic limit somewhere near 35 000 pounds, but that it must be measured not from the zero point of strain each way, but between the extreme strains, algebraically considered. I have also believed that what some experimenters claim to be a raising of the elastic limit of a bar by strain is simply a shifting of this

elastic range. If this belief is the true one, this whole doctrine of the fatigue of metals falls to the ground, and with it all those theoretical formulas deduced from such a theory.

Another argument made by the authors for this form of formula is the old American practice of allowing a larger "factor of safety" for live loads than for dead loads.

A careful examination of the evolution of the original Launhardt formula will show that the tendency is fast bringing the formula back to an identical form with the old American practice, to allow double the unit strain for dead loads that we do for live.

This is in accordance with my own views and the principle I have adopted in my recent bridge specifications. The reason for this practice is not, however, based upon any faith in the fatigue of metals, but upon the relative uncertainty of the loads and the strains induced. We can very exactly determine the dead load of a structure, and can have considerable faith in the strains determined therefrom; but for the live load there is much uncertainty as to its amount, and also as to the strains induced by this live load in rapid motion over a structure. We are, therefore, justified in accepting a "factor of ignorance," and with one-half of the allowed strain for dead loads.*

I am just reminded by a member that I have not made myself plain in regard to the reversal of strains in a bridge structure.

As long as we do not work our material by any manner of stress beyond the range of elasticity, I do not consider it can be injured, but of course we still have the doubt in regard to our live loads and their action. We should therefore keep well within the actual range in proportion to our ignorance of the absolute strains produced upon any members of the structure.

The reversal of strains therefore cannot hurt a bar with this proviso.

I do consider it very important to provide for extra strong con-

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*NOTE (added by letter).—Since making the above remarks, I find in *Engineering*, November 25, 1887, the following: "Professor Bauschinger has made a series of experiments on the alternation of the elastic limit, the experiments extending over several years. He concludes that "the elastic limit in tension is in general very different from that of the material when subjected to compression, and artificially raising the elastic limit in tension causes the limit in compression to be decreased, and this may even pass through the point of zero stress. In other words, a bar of steel or iron has two elastic limits, and whatever position these occupy on the scale of loads, the range between them is nearly a constant quantity. By alternately stressing a bar in tension and compression just beyond the elastic limits, these after a certain number of repetitions occupied positions equally distant from the point of zero load, and the limits thus obtained are called by Bauschinger the natural elastic limits of the bar. It was then noted that the stress corresponding to these limits sensibly coincided with that found by Wohler as the limiting stress to which a bar could be subjected to alternate tension and compression. It would thus appear that a bar will bear an indefinite number of repetitions of stress, provided the range of stress does not exceed the elastic range mentioned above."

This is a very strong confirmation of the views above expressed, and which I have held for more than fifteen years.

nections where the strains can be reversed. The alternate action upon the pins, rivets and other connections, are much more injurious in comparison to the same equivalent strain in one direction. In rivet connections, owing to this alternate action tending to work them loose, I think we should allow a greater number than where they are only pulled in one direction. The same would also apply to other connections where there will be more or less lost motion.

Mr. EMERY.—Mr. Cooper's remarks recall my original reading of the experiments. My recollection at present—and I would like to be corrected if wrong—is that they did not succeed in breaking a member, under reverse strains, until it was loaded by the method of computation up to or beyond the elastic limit. The ridiculousness of formulas extending within elastic limit, when based upon experiments without, is warranted by the remarks of Mr. Cooper. His explanations coincide with my remarks in regard to the intermittent application of reverse strains. When there is time for the metal to rest it assumes its former state; so, even if it be strained up to the elastic limit in tension and then be allowed to rest, it can safely afterward be carried to the elastic limit in compression. When, however, the strains repeatedly alternate from plus to minus, the algebraic difference must be taken, giving the numerical sum of the two, and the evidence seems to be that such sum cannot, for continued repetitions of strain, ever exceed the difference between zero and the elastic limit in either direction; but periods of rest between the intermediate applications of reverse strains change the rule entirely. The effect of flexing metals beyond the limit of elasticity, without rest, is readily seen with a material like brass, which hardens each time it is so bent. It is well known that such material cannot be bent back and forth in the same place many times without breaking. The rails of railroads, however, which must at times be strained as high on the outer fibers as 25 000 pounds per square inch in each direction, under the passage of a heavy locomotive on a yielding road-bed, rarely break, simply because the material has a period of rest between the passage of trains, and, at some hours of the day, very long periods of rest, so that on the whole the rails periodically resume their normal condition and are prepared for the next series of abuses.

It is well known that we must prevent motion. As soon as the particles are allowed to move, the jars may bring the strains beyond the limit of elasticity and an actual set take place, which, after a series of repetitions, will cause failure. Persons here present will recollect that in one of Mr. Metcalf's papers it is stated that different materials were tried for the connecting rods in a machine running at a high speed in which the weight of the parts brought a bending action on the rods. It was found eventually that high steel stood much better than any other material, the reason being that it was stiff enough to prevent the actual motion of particles, and there being no sudden jars, there was no danger

of rupture. It should be borne in mind that the strains on the outer fibers of bars subject to transverse strains are never as high as shown by formulas, for the reason that the fibers between the same and the neutral axis can, on account of the bending of the bar, take up a greater portion of the load than shown by the customary formulas, on the principle set forth in my paper several years ago on "The Transverse Resistance of Materials.

WM. H. BURR, M. Am. Soc. C. E.—Aside from the general character of a work of this magnitude, the excellence of its general design and the skill and energy displayed in the erection of these parts requiring false work over a river subject to great and violent changes of stage during the most dangerous season of the year, all of which reflect great credit on the engineers and contractors for the work, those portions of the paper outlining the results of special treatment of the steel members at different stages of their fabrication are of unusual interest and importance at the present time.

The conclusions to be drawn from the regular tests are only such as are usually reached in the ordinary testing of the material for steel plates and angles. The ingot tests of $\frac{3}{4}$-inch rounds do not give results essentially different in any respect from those of specimen tests of the rolled material, provided always that the amount of work expended on the material is essentially the same in the two cases.

The elastic limit runs a little over six-tenths the ultimate, and the final contraction is roughly twice the final stretch, approximate averages being considered. The exceptions to the latter ratios confirm the general principle preceding, since they occur in connection with the tests of eye-bar steel. In the case of this material, the steel of the bars naturally received much less work than that of the $\frac{3}{4}$-inch rounds; hence the elastic limit of the latter in general considerably exceeds that of the former, while the ultimate resistances are not very different. The differences between the elastic limits decrease as the bars become smaller and thinner.

The final contractions and stretches are about the same for the $\frac{3}{4}$-inch rounds as for the specimens from the bars. The effects of annealing are well shown in the tests of full sized eye-bars, which give an appreciable although not great decrease of elastic limit and ultimate resistance below the values yielded by the specimens taken from the bars before annealing. The final reduction is but little if any below that of the test specimens, except in one or two special cases, but the final stretch is of course much less.

The special tests, however, on shearing and cold hammering, and incidentally those on steel cast clevises, are of more than ordinary interest and importance. In noting the effect of these operations the grade of steel used is to be particularly observed, for there are good reasons to believe that the operations indicated, as well as some others

incidental to the production of finished bridge members, and which are usually excluded by specifications, may with propriety be employed with mild structural steel used under compression, but should be carefully avoided with a higher grade of metal, however it may be used.

In this paper, for example, on pages 165 and 166, the sheared edge specimens with edges not planed belonged to metal varying from 66 000 to 75 000 pounds in ultimate tensile resistance, yet the elastic limit and ultimate resistance were not appreciably affected. The final stretch and reduction, however, were reduced 35 to 50 per cent. This would be a matter of little consequence in a compression member, but would scarcely be permissible in tension. In 60 000 pound steel the effect of the shears would be still less, while in the 80 000 pound metal used in this structure it would be materially increased and could not be ignored.

The effects of hammering steel plates, as shown on page 167, are far less than we are led to believe would result, if we observe the great care with which hammering is excluded by specifications from shop processes. In fact, it is very difficult to draw from these tests the evidence of any deleterious effects resulting from the hammering of specimens. Some were hammered while hot and then cooled in ashes; others were hammered while cold, some of them being subsequently annealed, and the remainder allowed to remain as left by the hammer.

Those hammered hot and cooled in ashes seemed to gain slightly in ultimate resistance and lose a very little in final stretch, while those hammered cold and untreated subsequently seemed to lose a little in elastic limit, which was a rather remarkable result.

The sum total of the twelve tests does not reveal any essential effect of the hammering, nor anything that could not properly be ignored in any considerations affecting structural design.

Nor could any appreciable effect of the hammer be discovered in the tests recorded on page 168. The annealing clearly reduced the elastic and ultimate resistances and increased the ductility, but it is impossible to detect any influence of the hammer.

Some recent experiences of the writer also point strongly in the same direction as the preceding tests, and confirm the opinion that mild structural steel may be hammered cold with impunity under easily fulfilled conditions. I had a $\frac{9}{16}$-inch plate 5 feet 9 inches long and 12 inches wide severely hammered with heavy sledges in the hands of three men. These men, operating together, pounded the middle of the plate from fifteen to twenty minutes, until that portion of it was completely covered and indented with hammer-marks and the plate very much bent out of its original plane shape. The end portions, however, were in their normal condition. The plate was now straightened cold and six strips 18 x $\frac{9}{16}$ x $2\frac{3}{8}$ inches cut from it. Two of these were from the end portions, untouched by the hammer and in a perfectly normal condition.

The other four were cut from the central portion of the hammered part and had sustained the heaviest hammering, so far, at least, as could be determined from the marks and indentations. Four of these pieces were then pulled apart in a testing machine with the results given for Nos. 1, 2, 3 and 4 in the following table:

RESULTS OF TESTS OF UNANNEALED STRIPS OF $\frac{9}{16}$-INCH STEEL PLATES.

All Test Pieces $18 \times 2\frac{3}{4} \times \frac{9}{16}$ *inches.*

No.	POUNDS PER SQ. IN. AT		PER CENT. OF FINAL		REMARKS.
	Elac. Lim.	Ultimate.	Stretch in 6-in.	Contraction	
1. Normal.....	39,200	68,500	33	53	
2. do	40,800	70,000	31	35	
3. Hammered.....	53,100	71,500	16	41	Failed outside of gauge marks.
4. do	46,200*	71,100	9	48	do do do
5. Hammered twice.	58,460	73,100	2	42	do do do
6. do do ..	58,460	73,500	3	44	do do do

*The elastic limit of No. 4 is shown too low in consequence of an accident to the machine at this point of the test. These elastic limits are not well defined for the hammered specimens and indicate only the points at which the scale beam ceased to be quite stiffly held off its rest.

Specimens Nos. 5 and 6 were then taken back to the anvil for a second severe hammering cold with the sledges, and their subsequent appearance showed that they had been most thoroughly abused. In some places the thickness was reduced nearly $\frac{1}{16}$-inch and numerous indentations were of the most violent character. The results of pulling these in the testing machine are shown in the table as Nos. 5 and 6.

Now the results of these tests show that the cold hammering without subsequent annealing materially raises the elastic limit and slightly the ultimate resistance, but decreases the ductility. The amount of the latter effect, however, is uncertain, as failure in every hammered specimen took place outside the gauge marks, between which the stretch was measured. In all cases the contraction is well up to that of the normal specimens. As near as could be observed the specimens were a little less marked by the hammers near one end at which failure took place, showing that the metal flowed easiest where it was hammered least.

The intensified effects of the second hammering are easily seen by the increased elastic and ultimate resistances in Nos. 5 and 6. But all the unreasonable torture to which these specimens were subjected did not at all injure it for duty in a compression member, if, indeed, it has

reduced its tensile capacity. This hammering was done with ordinary sledges, but in no case was a crack started, and that fact undoubtedly explains the process of the tests. If steel plates are straightened cold, however, it should always be done with a "flatter," in order to avoid the possibility of starting any crack.

I also had some double counter-sunk steel rivets driven so as to produce the plates shown in the adjoining sketches. Two plates were riveted together with $\frac{7}{8}$-inch steel rivets, as shown in Fig. 1, with counter-sunk heads on both sides.

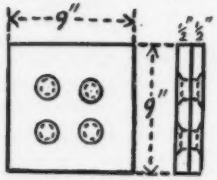


Fig. 1.

Two joints of 3 by $\frac{11}{16}$-inch plates, with $\frac{15}{16}$-inch double counter-sunk steel rivets, were also made, as shown in Fig. 2.

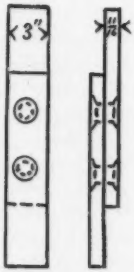


Fig. 2.

All these counter-sunk heads were driven by hand and hammered until and after they were completely black, or, as the workmen say, "until there was no heat in them."

The rivets shown in Fig. 1 were then driven out by placing against the driven head a hammer with a round nose less in diameter than the shaft of the rivet, and sledging it until the rivet yielded. Persistent and heavy blows given by two men striking continuously for about ten

minutes were required for each of three of the rivets, and the fourth one was acting similarly when the operation, about half completed, was suspended for the purpose of chipping off the head in order to discover in this manner, if possible, any evidence of brittleness. The heads of the rivets driven out presented the appearance in the sketch, Fig. 3.

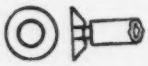


Fig. 3.

The round nose of the hammer sheared its way through the head and forced a purely tensile failure in the ring around it. Every rivet head showed extraordinary toughness, and not the slightest evidence of brittle, granular or crystalized material, and developed a resistance far beyond that of the best wrought-iron.

The fourth head showed in chipping just as much toughness and freedom from granular character as the others.

The joints shown in Fig. 2 were pulled apart in a testing machine. The first failed by shearing the rivets at about 57 000 pounds per square inch of the original $\frac{1}{4}$-inch section. The second broke in detail at a total stress of 68 000 pounds, by first shearing one rivet and then pulling the head off the other, in consequence of the large leverage afforded by the thickness of the plates, very much in the manner shown in Fig. 3, if the central case were not punched out. The fracture was wholly a tensile one, and purely silky in character. In none of these cases was there the slightest evidence of a granular character or of any deterioration whatever from hammering the rivet in driving until and after it was black.

While the number of these special tests is not large, they indicate, as do those of Mr. Moulton's paper, that mild structural steel will sustain far more torture cold without injury than is frequently supposed, provided always that no cracks are formed in the manipulation, and these experiences show clearly that they are not easily so formed and may be readily avoided.

F. W. Skinner, M. Am. Soc. C. E.—There is one point in the paper regarding which I have a record that may be of interest.

As I remember the paper, an objection made was, that in proportioning the lengths of main diagonals for camber an objectionable difference would arise between the lengths (normally equal) into which the middle pin divides the members.

A parallel instance is that of the St. John's Railroad cantilever bridge in New Brunswick, built by the Dominion Bridge Company, Montreal, P. Q., in whose service I had charge of computations and drawings. I

don't remember the exact dimensions, but there were two unequal armed cantilevers of six and eight panels respectively and a 100-foot center truss, making a total span of about 475 feet; it was a through bridge with towers 60 and 80 feet high, cantilever diagonals, double intersection.

A camber of 6 inches at ends of cantilever arms under dead load was required, provided it came within certain limits when under train load. The deflections due to distortion of members were figured, and then the decrements calculated that would give the members, when distorted by the given load, the required camber length.

The distortion calculations were based on values of moduli of elasticity obtained from tests especially made upon the steel used, those in compression from full sized columns tested at Watertown Arsenal, and those for tension from small pieces broken at the works, both on Emery machines.

The maximum difference between the lengths of bars of the same diagonal was certainly not above $\frac{1}{16}$ inch, and I have a very strong impression that it was almost exactly $\frac{1}{32}$ inch; at any rate it was decided to neglect it in the shop, and the work went together entirely satisfactorily and camber did not vary more than 10 per cent. from what was computed.

MACE MOULTON, M. Am. Soc. C. E.—I will preface my closing remarks by stating that I was not called in to assume charge of the superstructure (as may be noted on page 120), nor were any details submitted for checking until a year after the closing of the contract, based on the specification and general plans, and was not consulted regarding either specifications, plans or contract; and whatever changes in specifications were made subsequently were necessarily such as should not materially increase the cost to the contractors. Whatever opinions I may have held as to the vital features of the specifications or design were not solicited or offered, and consequently my duty was to see that the best structure possible to get under the existing contract was obtained.

The result is certainly far from being a discredit to either the designers or the builders, and may properly be considered a first-class structure. Mr. Schaub deplores the fact that no discussion was offered regarding the general design. The skeletons of original and final designs give data for any one interested enough in the matter to go into it to see the relative economy of the two.

The specifications, in using Launhardt's formula, certainly allow high stresses in some members with small proportion of live load, but also provide well for members first affected by the live load by adding percentages.

I do not feel called upon to defend the use of Launhardt's formula either with his values for "u" or those given in the specifications, and I have never designed a structure under it unless so instructed by the

purchaser. I think, however, that the prevalent deprecation of the great labor involved in its use is uncalled for. We are certainly nearing the time when all will admit that we should treat the live and dead loads separately, either to different factors of "ignorance," as Mr. Cooper calls them, or to some other application, to cover the effects of impact and repetitions of stress, and having the stresses calculated separately it is easy to place them in whatever relation seems most exact.

I studied the record of Wohler's and Spanenburg's experiments carefully some years since, and did not become a convert to the formula as Launhardt made it either in his value of "u" or in the co-efficient in the denominator. Regarding the loads used in the specifications which Mr. Wilson considers too low, I would say that in getting together the test train, I was able, by waiting two or three days, to get the engines corresponding to the diagram given, but no railroad running into Louisville could furnish a train averaging 2 240 pounds per track foot.

The projectors of the enterprise needed a bridge badly, and they built what they considered would answer their requirements and at the same time come within the appropriation. Mr. Schaub objects to the use of the close driven pin at the intersections of the diagonals, and makes some statements regarding the condition of compression members at points so pinned that I am unable to discuss for want of actual knowledge regarding the true state of affairs. I regret that he has not presented his new facts to accompany his assertions so that we might learn exactly how the case stands. In construction, I was assured that the intersecting pins were so located that the compression members should be least distorted when spans were fully loaded, *i. e.*, when camber was nearly taken out and members under nearly maximum stress. I had no opportunity to go into the calculation at the time, but have great confidence in the accuracy of the engineer who adjusted the matter. The contract was based on design with intersection pins, and the members were calculated on the basis that the post was held at the pin and as flat-ended at that point with a length equal to the distances between pin centers.

The tests on posts were made long after every feature of design was settled, and were in no sense used to determine points in the design, but were made for the purpose of ascertaining what they would stand and how they would act in the testing machine. The test on compression steel post proved by its slight set after stresses sufficient to distort rivets in splice that we are getting satisfactory compression joints from the rotary planer. I don't remember seeing a record of a spliced test post heretofore.

I do not agree with Mr. Schaub in wishing we may ever get to the point of ceasing to ream or drill rivet holes. The injury to the hole from punching is certainly there, even in iron, as experiments have plainly shown; and in steel, of whatever grade, the injuries will, in my opinion, seriously impair the strength of the piece, owing to the ten-

dency of every crack, though infinitesimal, to start a fracture. I have often noted the fact that pieces subjected to breaking stress would start a facture at some slight scratch or punch mark, and in some cases the fracture would occur at a point where its sectional area exceeded that of some other place in the piece.

Regarding the planing the edges of plates: The specifications called for universal mill plates, and there was no case in the construction of the entire structure where any other plate was put in to take direct stresses. In case of shearing off corners in compression members, such as in ends of posts or ribs of shoes, both beyond the pin, I did not ask them to be planed. In cases of links made of plate-steel or any other small pieces, they were, if sheared or punched out, put into the annealing furnace and annealed with the eye-bars.

I do not remember a single case where it was considered necessary to plane the edge of a plate on account of its having been sheared. The tests in sheared plates were made for the purpose of adding to our stock of information on the subject. The points which Mr. Wilson brings out as not being mentioned in specifications were all properly attended to in the working up of the designs. The specifications were so long that only extracts were given in the paper, several points which are common to almost every good specification being omitted. The lateral stresses were calculated under a live load, but its effect was smaller than usual, as the train must be nearly covered by the 8-foot high close fence between track and highway on either side.

I cannot inform Mr. Strobel why the pine plank were placed uppermost, except that it was so settled in the contract. Regarding the connection of floor beams to bottom chords, they are screwed up tight against the chords, and a plate was in every case put on the lower side of the chord, through which and the top splice plate of floor beam the lateral pins passed. The rivets in the connection of these plates to the chords were in all cases proportioned according to the longitudinal component of the lateral stress at that joint.

I am sorry to note a tendency to make light of experiments on full size parts in the the testing machine shown by Mr. Schaub regarding the posts, and Mr. Seaman in case of upset ends. In my opinion the more we test the more we know, and if we cannot get stresses near the elastic limit of the parts in the structure, the next best thing is to put the piece in a machine which will break it. We can certainly get some idea as to what to expect of a member when placed in a structure if we get tests enough of similar members in a testing machine. Now in case of steel lateral bars I took particular pains to have a test made of one of the regular bars, selected at random, because I had no knowledge of upsets on steel bars having been tested before, and I think that this structure was the first in which steel upsets were used.

Great pains were taken in shaping the upset dies to avoid sudden

changes of shape, and the result of the test (although only a single one) was very satisfactory. Roughly cut or poor threads of any kind were not passed by inspectors. As the steel was only used in very large bars (for lateral stresses), the upsets and the threads cut in them were very much in excess of the usual sizes, and their use was amply justified.

Information asked for by Mr. Wagner regarding shop manipulation will be found on page 140. The clevises were cast from crucibles with a head of metal about the same as would be obtained in an iron casting of the same size, and subsequently thoroughly annealed. In response to Mr. Wilson's request for my deductions from tests, I should say that the only point not already touched upon is the important one of how we shall inspect steel with a view to its acceptance or rejection. My judgment is based not only on tests made for this structure, but on observations in and about the mills, and of other structures under contract at the same time and later, the specifications for which differed materially from those of this bridge.

1st. Settle on what is desired as to the physical qualities of the steel for the purpose for which it is to be used.

2d. Buy material with those physical qualities, the same to be determined by tests from the finished material, taking not crop ends, but good material for the tests. Steel manufacturers can produce structural steel of any desired grade, and, as they know more than any one else about the amount and effect of work on the steel, they must be allowed to make their steel as they like, provided they produce the desired result. That they can get results to suit the purchaser was forcibly impressed on me on being shown tests two years ago under a specification for a very large quantity of varied material which required that the finished plates and angles should have the same physical properties as the ¾-inch round ingot test-pieces within narrow limits. The manufacturers did it, and that, too, with Bessemer steel; and if they could do it then they can do it again, and hence I think a buyer may with justice expect to get what he is paying for and not be asked to accept something which simply was once like what he demands, but which now, owing to variations in work put upon it, may vary 10 or 15 per cent. from what is designed to use in the structure. The lower the grade of steel called for, the more certain are the results and the fewer rejections liable to occur, and hence the cheaper for the mill and, of course, for the purchaser. Except in large structures the difference in favor of quite high steel is small, and there is where it seems reasonable to exercise the greatest caution in the use of the high grades.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

366.

(Vol. XVII.—October, 1887.)

TRIPLE THERMIC MOTOR: DESCRIPTION, OPERATION AND RESULTS OF A SINGLE EXPANSION, NON-CONDENSING STEAM ENGINE, SUPPLEMENTED BY THE EVAPORATION OF THE BISULPHIDE OF CARBON AND EXPANSION OF ITS VAPOR, AT BRUSH ELECTRIC LIGHT COMPANY, CLEVELAND, OHIO.

BY CHARLES H. HASWELL, M. Am. Soc. C. E.

READ AT THE ANNUAL CONVENTION JULY 1ST, 1887.

CONSTRUCTION.

First.—An ordinary horizontal and cylindrical fire tubular boiler.

Second.—A tubular generator in form of a cylinder boiler set horizontal in which the material of vaporization, known as bisulphide of carbon (formula CS_2), is vaporized, having attached in the vapor space an ordinary perforated dry pipe.

All of which is inclosed in a shell having a diaphragm plate between the outer and inner shells at both sides and at one end, thus forming an upper and lower chamber around it. The opposite end is inclosed with a deep disc or bonnet, thus forming a communication between the lower and upper series of tubes, for the proper circulation of the steam with which the CS_2 is vaporized.

Third.—An ordinary horizontal non-condensing jacketed steam engine.

Fourth.—Conduit or vapor-pipe, steam jacketed by being concentric

with one extending from the generator to the cylinder of the engine, the jacket of the conduit communicating with the jacket of the cylinder, and from thence the condensed steam is led by a pipe to a steam trap communicating with the feed pump of the boiler.

Fifth.—An automatic regulator or pressure reducing valve, for controlling the admission of steam to the shell surrounding the generator; operated by the pressure assigned to the generator; thus holding the vapor pressure uniform, by admitting more or less steam to the shell as the variation of the load on the engine may require.

Sixth.—An automatic reducing valve for controlling the pressure in the jacket around the vapor conduit and cylinder.

Seventh.—A heater having within it a coil pipe, through which the condensed CS_2 is forced back into the generator.

Eighth.—An ordinary surface condenser.

Ninth—Three ordinary and small independent steam pumps, and a connection to a water main or centrifugal pump, from which the water of condensation is obtained.

The generator, *i. e.*, the vessel in which the material of vaporization CS_2 is vaporized, and by which the expansive force is obtained wherewith to operate an engine for motive power, is charged with it to a little over one-half its capacity.

Steam, previously generated in a boiler or primary motor, is led by a pipe to and through the automatic regulating valve, where it is reduced in pressure and consequent temperature; thence to the generator through a perforated pipe between the shells below; thence flowing around the lower half of the generator shell; thence through the lower series of tubes; thence through the upper series; and thence between the shells above; thus circulating through, and radiating heat to the entire surface of the generator: CS_2 taking up the latent heat and a portion of the sensible, thus the steam is condensed and gravitates to the bottom of the outer shell; from thence to the boiler feed pump (being one of the three referred to), with the condensation from the jacket of the conduit and cylinder, as delivered through the steam trap; and from thence returned to the boiler.

Steam is also admitted through the reducing or regulating valve, to and through the jacket of the conduit to the jacket of the cylinder, where it is restricted to a reduced pressure, and as it is at a temperature due to this pressure, it is at a temperature in excess of that surrounding the generator, thus imparting an increased temperature to the vapor and effectively superheating it in its course to and in the cylinder of the engine.

The vapor generated from CS_2 in the manner described, is delivered through the dry pipe to and through the conduit to the cylinder of the engine, and from the greater temperature surrounding the conduit and cylinder, the vapor is increased in temperature and consequent volume from its admission into the conduit and cylinder, until it reaches the point of cutting off, after which it continues to take up the surrounding heat, thereby supporting its expansive force until it has completed its function in the cylinder.

The exhaust vapor from the cylinder passes around a coiled tube in the heater referred to; thence to and through a surface condenser, from which it is drawn off by the second of the three pumps and delivered into an auxiliary condenser (through the tubes of which the circulating water is first introduced from the main; from thence through the tubes of the condenser; and thence discharged, as in a sewer), thus attaining perfect condensation of the vapor.

Any air drawn from the condenser by the air pump passes through a vessel termed the washer (which is partially filled with water) and thence to a waste pipe.

The liquid CS_2 gravitates from the auxiliary condenser to a reservoir. From thence it is drawn by the third of the three pumps and delivered through the coil in the heater (where it absorbs heat from the exhaust vapor on its passage to the condenser); thence to the generator, where it is again vaporized.

An entire plant designed for the development of the practicability and economy of this design of engine has lately been constructed for the Brush Electric Light Company, at Cleveland, Ohio, and on the 31st of May a test of it was made under the direction of Mr. Isaac V. Holmes, representing the Light Company.

Elements of the Plant.

An ordinary horizontal fire tubular boiler set in masonry having a grate surface of 16.5 square feet and a shell and tube surface of 225 square feet.

Combustion, natural draught. Coal, anthracite.

An ordinary jacketed non-condensing engine, single expansion, having a diameter of cylinder of 14 inches and a stroke of piston of 36 inches.

Generator, having a diameter of 54 inches, a length of 15.25 feet and a steam heating surface of 1 550 square feet.

Surface condenser having tube surface of 1 000 square feet.

The operation of the engine was contined without interruption for a period of five hours, which, inasmuch as that period involved the cleaning of the fire in the furnace of the boiler, was held to afford full time for a test of the operation and the effects of the elements under investigation.

RESULTS.

Pressure, steam—Boiler	75.8 pounds.
" " Shell	15.3 "
" vapor—Engine	76 "
" " Mean, by indicator	31.35 "
Water evaporated	5.71 cubic feet.
Revolutions per minute	100
Vacuum	9.85 pounds.
Coal consumed	600 "
Horse power indicated	86.64.

NOTE.—Pressures are given in pounds mercurial gauge, and temperatures in degrees of Fahrenheit.

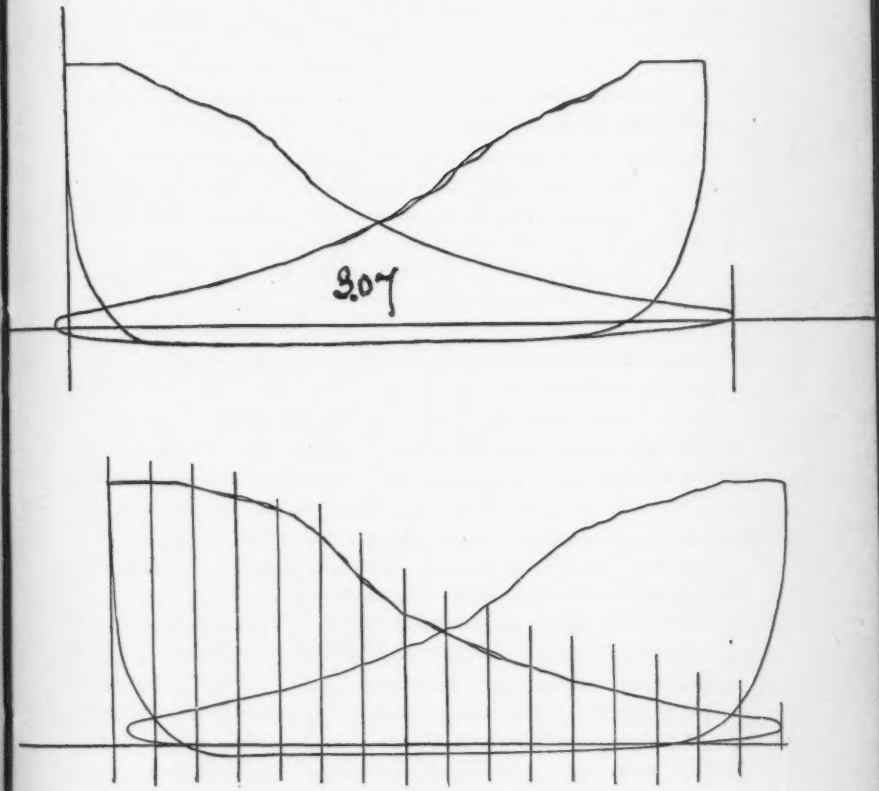
From which it appears that steam at a pressure of 75.8 pounds per square inch, passed through the automatic regulating valve to the shell surrounding the generator at the reduced pressure ef 15.3 pounds, due to a temperature of 250.4 degrees, produced a vapor in the generator of 76 pounds.

Sum of sensible and latent heat of steam at one atmosphere	1 178.1 degrees.
Sum of sensible and latent heat of CS_2	274.4 "
Latent heat of steam at one atmosphere	965.2 "
Latent heat of vapor of CS_2 at one atmosphere	156.4 "
Sum of sensible and latent heat of steam at 45 pounds (the pressure of the steam surrounding the vapor in the conduit)	1 206.6 "
Sum of sensible and latent heat of the exhaust steam at one atmosphere, as defined by the indicator cards	1 178.1 "

Hence, 1 206.6 minus 1 178.1 equals 28.5 degrees expended in the cylinder.

Sum of sensible and latent heat of CS_2 vapor at 76 pounds pressure, and superheated by steam at a temperature of 292.3 degrees (45 pounds) is	443 degrees.
Sum of sensible and latent heat of CS_2 at one atmosphere, that being the temperature of the exhaust, is	274.4 "

AREA, 3.07



CARD ILLUSTRATIVE OF EXPANSION OF THE VAPOR.

PLATE XXII.

Hence, 443 minus 274.4 equals 168.6 degrees expended in the cylinder, and 168.6 divided by 28.5 equals 5.916.

Therefore, the relative theoretical value of the vapor of CS_2 as compared with that of steam is as 5.916 to 1; or, in other words, the heat admitted to the shell around the generator, additionally heated by the excess of that of the steam around the conduit and cylinder, will produce an elastic vapor in the cylinder of an engine 5.916 times greater than if steam alone was used.

Or, steam at 15.3 pounds pressure generates a vapor of 76 pounds pressure, which when additionally superheated by being enveloped in steam at 45 pounds, its volume is increased and its pressure of 76 pounds is fully maintained at the cylinder of the engine.

In the test, the elements of which are here recorded, the consumption of coal per hour was 120 pounds.

Indicated horse power, 86.64.

Hence 120 divided by 86.64 equals 1.385 pounds per indicated horse power per hour.

The volume of water at 50 degrees (the ordinary or mean temperature of condensing water) required to condense steam at the temperature of that of the vapor which passed through the heater (212 degrees, as determined by the indicator card) and deliver it to the boiler at the ordinary temperature of 100 degrees, is as 22.95 to 1, the volume evaporated.

The volume under like conditions for the condensation of the vapor is but as 3.67 to 1, to the volume evaporated.

Hence, when CS_2 is compared with steam, a less area of condensing surface and less volume of condensing water is required, both of which conditions involve an economy in the cost and endurance of an engine.

Reviewing then the elements submitted, it is presented, that by the use of the vapor of bisulphide of carbon in a vessel connected to an ordinary steam boiler and engine, condensing or non-condensing, an elastic vapor can be attained greatly in excess both in pressure and volume of that of the steam that generated it; and, as an evident and infallible consequence, both increased power and economy of fuel are attained with less boiler surface and consequent wear.

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(Vol. XVII.—October, 1887.)

THE DAVIS (CREVASSE) LEVEE.

By SIDNEY F. LEWIS, M. Am. Soc. C. E.

READ JULY 7TH, 1886.

On March 8th, 1884, at about one o'clock in the morning, a break occurred in the Davis Levee, situated on the west bank of the Mississippi River, some twenty-three miles above the City of New Orleans.

The crevasse, in the official report of the Board of State Engineers for the years 1882–83, and to April 20th, 1884, is said to have been caused by the washing out of loose earth in an imperfectly refilled rice flume cut in the levee. The levee at this point was upwards of eight feet high, and the water in the river on that date was within eighteen inches of the top of the levee. The gauge at New Orleans* read one foot one inch below high water mark of 1874.

The position of the break was near midway in a bend of the river, where the current impinges directly against the bank. The width of foreshore about sixty feet, with a depth abrupt to it of over fifty-eight feet at a low stage of the river. This sixty feet of foreshore was cut away in a short time.

* The zero of river gauge at New Orleans was changed on November 1st, 1885, from high water of 1874 to low water of 1876. This change makes the readings 16.2 feet lower than those made previous to November 1st, 1885.

On March 27th the dimensions of the crevasse was found to be as follows:

River below flood stage	0.5	feet.
Width of outlet	557	"
Area of outlet	11 640	square feet.
Reduced mean velocity per second	12.9	feet.
Discharge through outlet per second	140 156	cubic feet.

After subsidence of high water it was found to be 1 400 feet wide.

In the incipient stage of the break an attempt was made to close it by the Texas and Pacific Railroad Company and Morgan's Louisiana and Texas Railroad Company, their tracks being submerged by the waters of the crevasse, but after considerable expense in labor and material their efforts proved futile. In a few weeks a section of country (as represented by the accompanying map, Plate XXIII) some thirty-three hundred square miles in area became an inland sea, separated from the river, as it were, by a green ribbon of levee. Sugar and rice plantations, with the many vegetable and fruit farms, for over sixty miles above the city, and to the limit of planting interests, some sixty-five miles below the city, were covered with water from three to five feet deep. It may be interesting to state that the U. S. steamer Patrol, a stern-wheeler 110 feet long, drawing over four feet of water, whilst in the service of gauging the crevasse, was carried through the break without injury, and was afterwards piloted safely across the overflowed country to Bayou La Fourche, and thence back to the Mississippi River.

The suburbs of New Orleans, Gretna, Gouldsboro and Algiers were mostly under water, with here and there isolated spots, which, by their favored positions or back levees, were able to keep the crevasse water out of the dwellings and yards.

The road-bed of the Morgan's Louisiana and Texas Railroad Company was damaged and submerged for forty-eight miles. The road-bed of the Texas and Pacific Railroad Company was damaged and submerged for over twenty miles. For a period covering more than four months these sections of roads could not be utilized, and passengers and freight had to be transported by boats to their respective termini above overflow.

The following estimated losses of the Morgan's Louisiana and Texas Railroad from Davis crevasse is transmitted:

"The loss in business is based on the decrease in net earnings, amounting to $693 972.48, which result was brought about by:

PLATE XXIII
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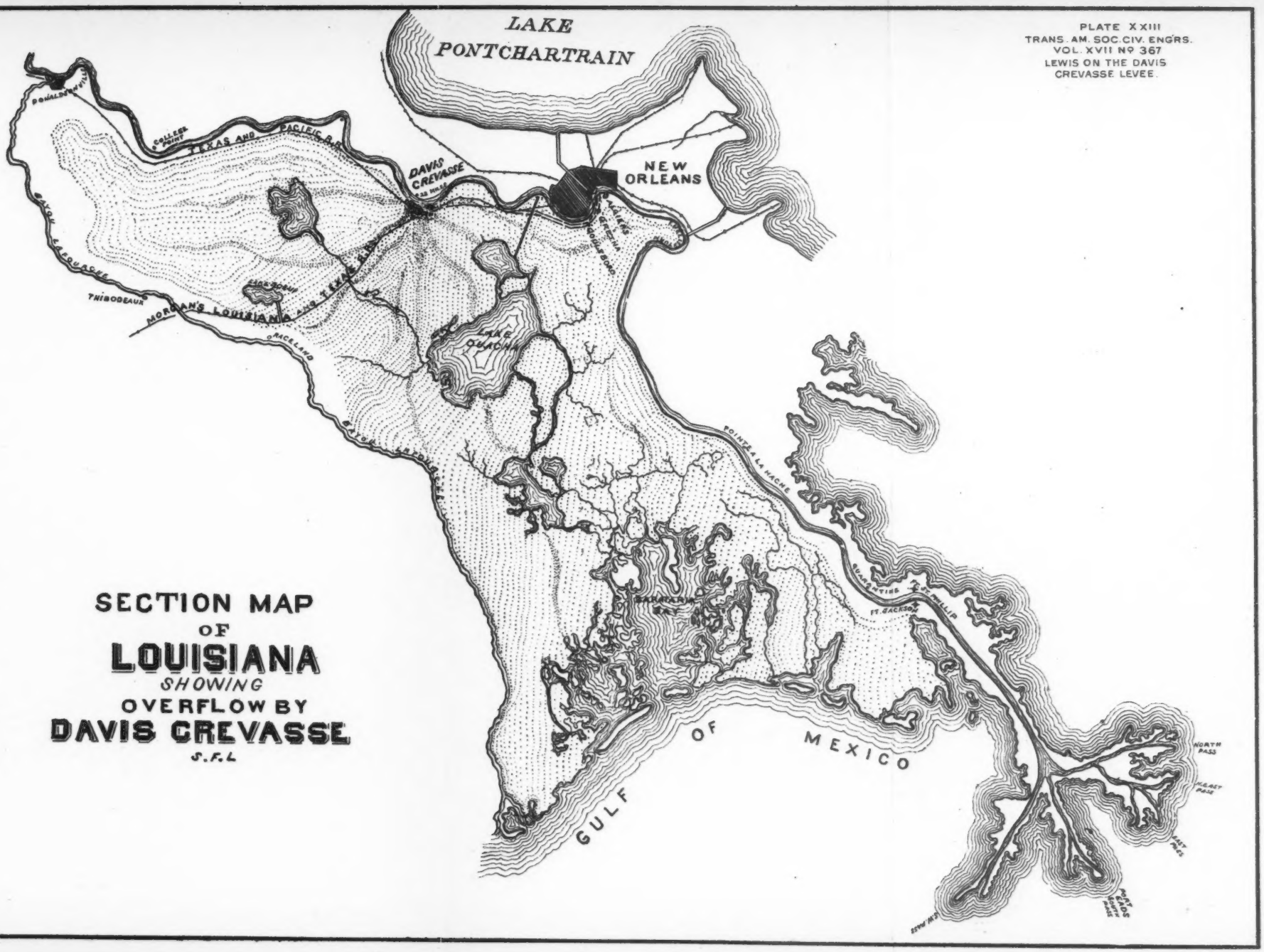
LEWIS ON THE DAVIS
CREVASSE LEVEE.



"*First.*—Increased cost of operation by roundabout and expensive routes.

"*Second.*—Diminished revenue.

"*Third.*—The small volume of business done after the reopening of the road, on account of the country tributary to it having been ruined.

"Trains stopped running on March 11th, 1884, and commenced running again on July 26th, 1884.

"It would not be out of proportion to estimate the cost of protecting the forty-eight miles, and of putting them in first-class order after the overflow, at five hundred dollars per mile, or $24 000.

"Money actually spent in attempting to close Davis crevasse, $22 500.

"It is impossible to estimate the losses incurred by the Texas and Pacific Railroad from this crevasse alone. On March 20th the Atchafalaya River overflowed its banks and submerged the track in the swamps, so that the New Orleans Division was not operated south of Bunkie from March 20th until August 15th, when the repairs of road-bed at Davis crevasse were completed, and the line operated through to Gouldsboro.

"Amount expended by Texas and Pacific Railroad in efforts to close the crevasse, including labor and material, about fourteen thousand dollars."

It was not until the latter end of August of that year that the water over the land and in the river had sufficiently declined to make instrumental surveys for a line of levee to inclose the break. As engineer in charge of that levee district, the surveys were intrusted to me. Two lines for a levee were located, one to circumvent the washout or gulch caused by the crevasse, as represented by the dotted line $A\ B^1\ C^1\ D$ (see Plate XXIV), requiring about one hundred and sixty-five thousand cubic yards of earth; and the one finally adopted and built across the channels, as represented by the line $A\ B\ C\ D$, three thousand eight hundred and one feet in length, requiring some one hundred and fifty-three thousand cubic yards of earth.

This line offered considerable difficulties in surveying, as the surface of the ground was very much honeycombed by the waters of the crevasse, and the channels at that stage of the river averaged sixteen feet deep.

On the decline of the river in August, the Texas and Pacific Railroad Company, whose track was nearest the break, had trestled over the channels of the crevasse in pretty much the same line as their original road-bed, and this trestle served the purpose of a base to arrive at an accurate survey and cross-section of the ground adjacent thereto. This line, after due deliberation, was adopted by the Board of State Engineers, not only because it was much the shorter of the two and contained less yardage, but that the levee on this line could the more easily and readily

be built, as the existing trestle of the Texas and Pacific Railroad Company was utilized for the construction of the larger sections of the embankment across the channels, by hauling earth in flat-bottom cars, using a steel plow and cable to unload them, whilst the approaches or wings could be built with wheelbarrows, and the construction of these different sections of levee kept in progress independent of each other.

With this purpose the following designs of sections for the embankment were devised and adopted by the Board of State Engineers. (Plate XXV).

Fig. 1.—Average cross-section of levee for upper wing or approach.
Fig. 2.—Average cross-section of levee for lower wing or approach.
Fig. 3.—Cross-section of levee for deepest portion of channel on river side of railroad trestle.
Fig. 4.—Cross-section of levee for deepest portion of channel on land side of railroad trestle.

On September 5th, 1884, a force of about one hundred able-bodied men began breaking ground and wheeling earth on the upper wing or approach *C D*.

On September 11th, 1884, a similar force was put to work on the lower wing or approach *A B*. Another camp of about eighty men later on were put to excavating a pit for loading the cars.

The approaches or wings, aggregating some thirty-eight thousand seven hundred and fifty cubic yards of earth, were completed about October 1st, 1884.

The camps were then concentrated on the curtain or main line of levee, which was partially built with earth hauled in flat-bottom cars, and then topped off with wheelbarrows.

In the months of January and the early portion of February, 1885, the lower Mississippi River attained an unprecedented height for that time of the year, averaging some two feet below the high water mark of 1874 at New Orleans.

During this period some three hundred and fifty feet in length of the embankment across the upper and deepest portion of the channels of the crevasse was in a precarious condition, as scarcely more than one-third of the earth required had been placed in position, and in consequence gave rise to great anxiety to the railroad authorities and other parties interested. The river subsided as quickly as it had risen, and subsequently during the spring floods never attained its February height. This uneasiness led the railroad authorities to drive a row of sawed piles twelve inches square and fifty feet long across the upper channel.

PLATE XXIV.
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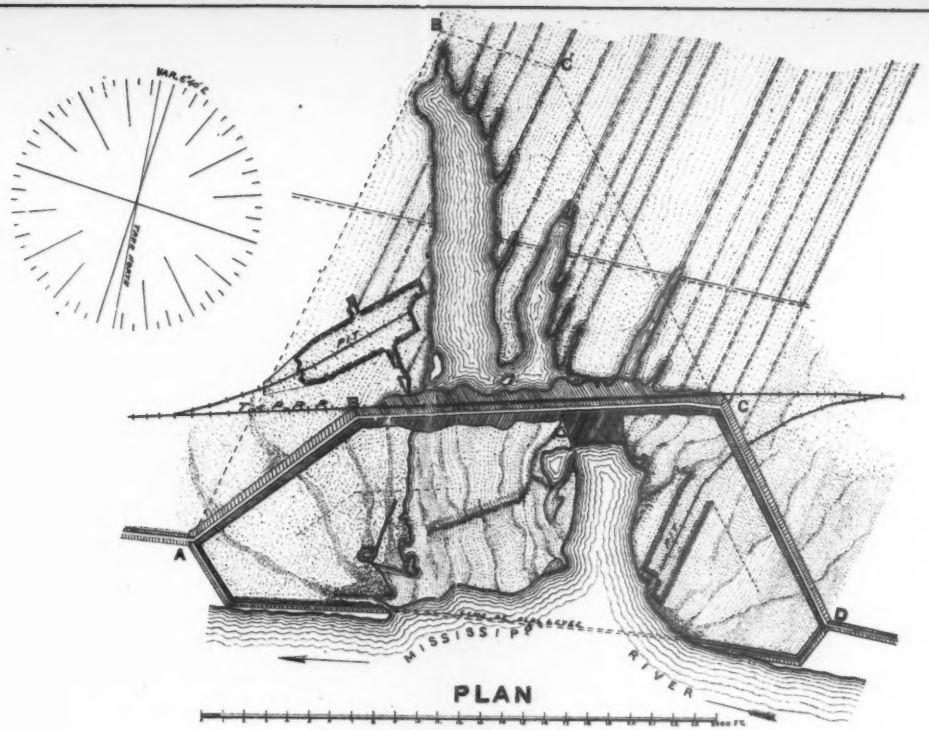
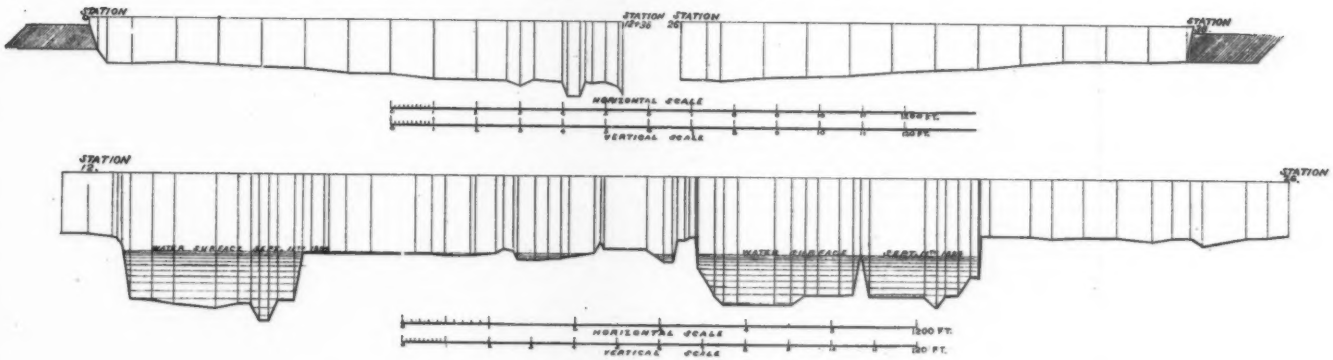


PLAN

PROFILES OF CENTRE LINE OF LEVEE



DAVIS LEVEE

ST. CHARLES PARISH. RT. BK.

LA.

S.F.L.

PLATE XXV
TRANS. AM. SOC. CIV. ENG'RS
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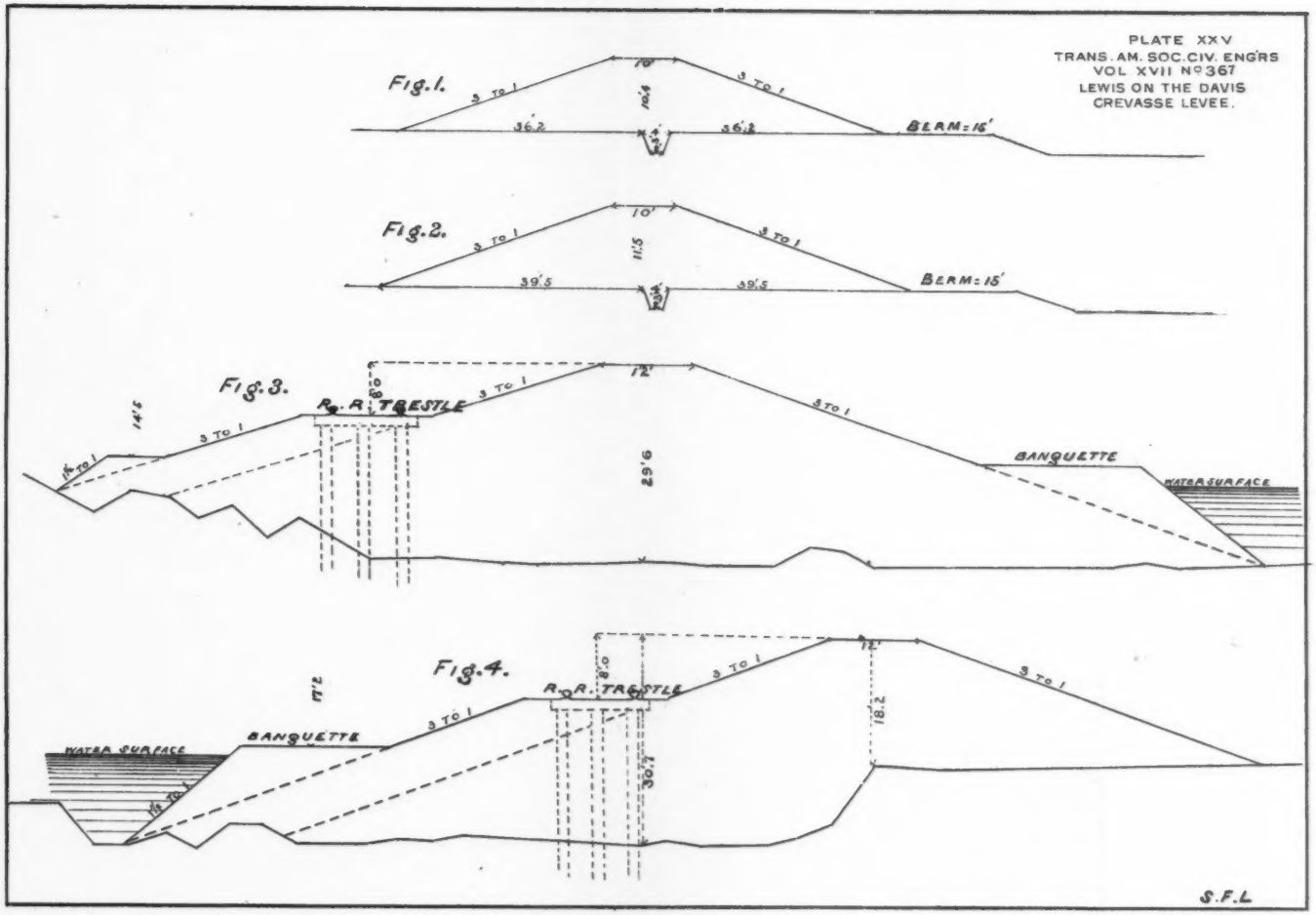
LEWIS ON THE DAVIS
CREVASSE LEVEE.



At the suggestion of the Superintendent and Engineer of the Morgan's Louisiana and Texas Railroad Company, they were driven on the river side forty feet from center of levee when completed, some fifteen feet in the solid ground, six feet from centers, and capped with an horizontal piece of twelve by twelve-inch bolted through the head of the piles.

Sections of the embankment across this upper channel taken on February 9th, 1885, showed the piles to be leaning. As the earth was placed in position these piles continued to give way, until finally they were completely overturned. A second row of round piles outside of these were then driven, which met with a similar fate. The river half of this section of the embankment, as referred to above, would crack and slide with a slow motion towards the river, the tendency and amount of slide increasing or decreasing with the fall and rise of the water, whilst the half section on the land side remained firm and steady with no disposition to change its slope of three to one.

In consequence of this it became necessary to keep a wheeling force of upwards of sixty men during high water season to keep the dump above the surface of the water in the river. When the river had sufficiently declined so as to leave this section of the embankment out of danger, it was deemed advisable to relieve the wheeling force, as it was presumed that the summer months would dry up and help this sliding portion of levee. It was also found that there had been over twelve feet of deposit from the river in the main channel of the crevasse.

In the following fall a force of about twenty wheelbarrows were put to work to complete this section of the embankment, it being estimated that some twenty-six hundred cubic yards of earth would be required to bring it up to grade.

On October 27th the sliding of this section of the embankment began to reshow itself; the wheeling force was kept at work to April, 1886, when it was relieved, as the embankment had about come to its bearings.

The cost of this levee up to date amounts to $38 145. It is probably the cheapest levee for its magnitude ever constructed in this State.

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(Vol. XVII.—October, 1887.)

REPLACING THE STONE TOWERS OF THE NIAGARA RAILWAY SUSPENSION BRIDGE, WITH IRON TOWERS.

By L. L. BUCK, M. Am. Soc. C. E.

READ NOVEMBER 2D, 1887.

It is not the intention in this paper to give a minutely detailed description of the old stone towers of the bridge, but merely such a brief description as will explain their condition, the causes which led to their replacement and the manner in which it was done.

The stone towers rested on the rock which forms the banks of the gorge spanned by the bridge. On the New York side they were about 90 feet in height, from the rock to the center of the cables, where they rest in the saddles. On the Canadian side, the rock being about 10 feet higher than on the New York side, the towers were about 80 feet high. Each tower, of a pair on either side of the river, had a heavy base of rock faced ashlar, extending to about the level of the upper chords of the stiffening trusses of the bridge, and the two bases were connected by an arch to support the railway track, between the towers, and allow of the passage of pedestrians and vehicles to the lower floor. The portion of each tower above the base approximated to the frustrum of a pyramid 15 feet square at the base and 8 feet square at the top. They were built of dressed limestone, laid in Thorold cement mortar. The top of each tower was entirely covered by a cast-iron bed-plate 2½ inches thick. On the upper surface of this bed-plate were cast three ribs, parallel to the direction of the cables, 2½ inches thick by 7½ inches high, and extending across the plate. The two spaces thus left be-

tween the ribs were 2 feet 2 inches wide, and truly planed on their bottoms and sides. In each space were ten cylindrical cast-iron rollers, turned to 5 inches diameter by 2 feet 1¾ inches long, laid close together, side by side, and transversely to the ribs. On each set of rollers rested a saddle of cast-iron, 5 feet long by 2 feet 1¾ inches wide at the base, the under side of which was planed to rest on the rollers. The upper portion of the saddle was provided with the usual U shaped groove in which the cable was laid with an easy curve. Through the upper portion of the saddle, between the base and U shaped groove, were cored nine holes, extending from side to side. These holes are mentioned here for the reason that they served an important purpose in securing the cables to the lifting apparatus.

The stone of which the towers were built was limestone, quarried near the site of the bridge. A few of the first stones laid at the bottoms of the towers were taken from the surface stratum and remained sound; but, for some reason not explained, all the remaining stones were taken from a stratum below, which would not bear exposure to the weather.

Sometime after the completion of the towers the defective character of the stone began to manifest itself. Irregular shaped pieces would become detached and cracks appeared in various parts of the surface. Painting was resorted to as a preservative, and was continued at intervals, as long as the towers were retained.

My first experience with the bridge was in 1877. At that time the surfaces of the towers were considerably disintegrated, and pieces had fallen off here and there. Most of the cracks were in every conceivable direction, but there were a number of cracks which took a nearly vertical direction, generally indicative of pressure being their cause.

But little attention was given to the towers at the time of their inspection that year, for the reason that the condition of other portions of the bridge was apparently of more importance.

New face stones had from time to time been inserted. But in 1880, while the work of rebuilding the suspended superstructure was in progress, the surfaces of the towers were in bad condition, numerous pretty large pieces having fallen off, and a good many more were loosened and ready to fall. At that time I noticed that, in external appearance, the most defective portions of the towers were those located about one-third the distance from the level of the railway floor to the top, and that the disintegration gradually decreased as the distance above and below this

point increased. This appearance could only be accounted for by supposing that the towers were being subjected to a considerable bending stress, which by opening the joints, cracking the paint and admitting water, coupled with the increased pressure at the surface, and the bad character of the stone, would fully account for it. Moreover this was the part of the tower subjected to the greatest bending stress.

The cause of this bending stress was the elongation and contraction of the cables between the towers and the anchorage, produced by live load stress and changes of temperature. This portion of the cable is as nearly straight as its own weight alone will allow the tensive stress to which it is subjected to make it; the deflection averaging about 15 inches. The maximum movement at the top of the tower, produced by the live load, is about $\frac{3}{8}$ inch, while that produced by temperature, between extremes, is a little over two inches. As these two act in concert only at the higher temperature, it is evident that the greatest movement* of the saddle at extreme high temperature would be about $1\frac{5}{8}$ inches from its normal position and toward the river, while in the opposite direction it would move from the normal position to that at extreme low temperature, about 1 inch.

In 1880 the greatest movement of the saddles with respect to the bed-plate was $\frac{1}{4}$ inch. The rollers were so situated as to be inaccessible excepting by cutting away the outer ribs of the bed-plate, but these ribs were the only guides to prevent the saddles from working laterally, and as events have since proved, it could not have been of any benefit.

After completing the suspended superstructure of the bridge, I was requested by the Board of Direction to report upon the condition of the towers and as to what ought to be done upon them.

My examinations had already convinced me that any plan for buttressing the towers or otherwise attempting to prevent their bending would prove abortive. If encased in sheet iron nothing could be known of the changes taking place within, and as the motion would still continue, it was very doubtful whether such covering could be kept water-proof.

The only plan that promised any success appeared to be that of cutting away the defective portions of the stone to various depths or till the sounder portions within were reached, and then reface the towers

* Since the above was written the movement for the present winter between + 40 degrees and zero has been $1\frac{1}{2}$ inches.

with new stones, thoroughly anchored to the interior portion, and cement and grout all interstices.

There appeared to be a possibility that if this work was well and carefully done, the new covering would possess sufficient flexibility to allow of the swaying of the tower without crushing the new stone. At that time it was supposed that the interior portions of the towers were sound.

My report favored this plan as an experiment, and it was decided to have the work done. The refacing was completed in the latter part of the year 1883; the last tower treated being the north one on the Canadian side.

During the summer and fall of that year I was in Portland, Oregon, and consequently saw very little of the interior portion of this last tower, as disclosed by the cutting away. On my return from the West I was informed by Mr. Patten, who had charge of the work, that this last one had two cracks in it of such a serious nature that they gave him considerable uneasiness. One of these cracks started at the middle of the top of the south side, and the other at the middle of the top of the west side. Both extended downward, gradually approaching the southwest corner, where they met at a point above the railway floor about one-third the height of the tower above that floor.

A close watch was kept upon the towers to detect any change that might occur.

In September, 1885, I inspected the towers. At that time the north tower on the New York side was apparently in fair condition. The south tower, on the same side, had opened some of its joints slightly, and two or three of the new facing stones were slightly cracked. On the Canadian side the south tower had opened some joints and cracked some of the new stones. A serious crack had also started in the old stones on the east face, beginning about twelve inches from the northeast corner. Thence it extended upward and toward the middle of the face, through three courses. This was more serious than the cracks in the new facing, for the reason that as the old stones were originally very large, such a crack must necessarily extend to a great depth.

The north tower was in a far worse condition than any of the others. Many of the joints in the new faces had opened; but, on the south and west faces, the joints had not only opened but many of the stones were badly cracked, and the southwest corner, at the point above mentioned, appeared about ready to fall out.

At this time, motion of the saddles on the rollers had about ceased. This could have been remedied by lifting the saddles, removing the old rollers, and by using a larger number of rollers of less diameter with new planed plates above and below them, and then by refacing the towers, they might be made to stand some time longer. But the expense for apparatus and refacing would be considerable, while the internal condition of the towers would still be uncertain. Moreover, all the rollers on top of one tower only covered an area of 3 feet 4 inches by 4 feet 7 inches equaling 17.18 square feet. The bed-plate could not effectually increase the area of distribution of the weight to more than 25 square feet. The maximum weight of about 900 tons resting on the rollers would cause a pressure of 36 tons per square foot on the masonry. As shown by sounding the plates around the edges with a hammer, the area given above as actually being pressed upon is certainly not understated. There was consequently a probability that (considering the character of the stone in the towers) the interior stones at the tops of the towers would be found unsound.

These considerations decided me to report in favor of replacing the towers with iron.

The following winter I prepared plans for the new towers, and the Board of Direction having decided to act upon the recommendation, the contract for the iron work was let to the Detroit Bridge and Iron Works.

The plans for the new towers, method of erection and of transferring the cables were according to the following description (see Plate XXVI).

Each iron tower is made up of four wrought-iron columns, braced transversely and longitudinally by wrought-iron struts and rods. Each column rests upon a limestone pedestal, founded on the rock and capped with granite. The outer portions of the old bases were cut away to make room for the new pedestals. The tops of each pair of columns are connected by a heavy web plate secured to each web of each column by angles and rivets. Their tops are covered by a wrought-iron plate truly planed to present a level surface when in position, thus forming a cap. On the caps of the two pairs of columns rests the main bed, built up of wrought-iron plates and angles. It is in plan 9 feet 2 inches long by 5 feet 3 inches wide and in height 3 feet 8 inches. Its weight is about 9½ tons. The whole of its top surface is truly planed. On it are laid flat, with no fastening, two steel plates, each 2 feet 2 inches wide, $\frac{7}{8}$ inch thick by 7 feet long, with both sides and both edges planed

parallel. These plates are laid with their long sides parallel with the direction of the cable. On top of the steel plate, and laid transversely, are eighteen steel rollers, turned truly cylindrical, with diameters of 4 inches and length between shoulders equal to the width of the steel plate. The ends of each roller have trunions which enter into holes in the side pieces of the roller frame. The side pieces of this frame are 1 inch thick by scant 5¾ inches wide, and have their inner faces and both edges planed so as to come into neat contact with the edges of the steel plates and shoulders of the rollers. Resting on the rollers is a cast-iron bed whose base is 7 feet long by 2 feet 4 inches wide, height 18 inches, and top 5 feet by 2 feet 2 inches. The top and bottom surfaces are planed parallel, and to the under surface is riveted a planed steel plate of the same dimensions as that under the rollers. By this arrangement the rollers are protected from water and, as far as possible, from dust. On this casting rests the old saddle. In addition to the bracing of each tower, each pair of towers is braced together transversely.

The transferring apparatus consisted of four cast-iron columns, two transverse and two longitudinal girders, six 125-ton hydraulic jacks, a quantity of wrought-iron shim plates and of fine quality of No. 8 cast-steel wire.

When the columns of the towers had been erected in place, the transverse bracing was put in immediately, but the longitudinal bracing could not be put in till the stone towers were removed. Consequently their place was temporarily supplied by clamping the iron work to the stone towers.

After the columns had been so placed and clamped, the temporary cast-iron columns were set up, one at each end of each cap, when their upper ends extended above the cable, and the space between a pair of the cast columns was such as to allow of passing the large bed between them. On top of each pair of cast-iron columns rested a transverse girder, and on the two tranverse girders rested the ends of the two lifting girders. When in position, each lifting girder was directly over and parallel to a saddle. The saddles were then secured to the lifting girders by passing the steel wire through the before-mentioned holes in the saddles and up over cast-iron blocks on the lifting girders. The wire was thus passed continuously around with three hundred and fifty turns for each saddle.

The six jacks were then set under the transverse girders and resting

on the caps of the wrought-iron columns, in the space through which the bed was to pass—three jacks under each transverse girder. The bed was then hoisted on the side toward the river and temporarily supported, close under the cables, by a wrought-iron frame, the end of the bed next to the tower resting on rollers to allow of its being easily moved. The cast-iron saddle beds, rollers and steel plates were then raised and conveniently placed on the staging, between the towers.

The six jacks were then operated simultaneously, thus lifting the transverse girders, which raised the longitudinal girders, carrying the saddles and cables with them. As the transverse girders raised, the iron shim plates were inserted, between their ends and the tops of the cast-iron columns, till a sufficient number of the plates had been inserted to keep the saddles clear of the old rollers, when the jacks had been discharged. As this required a considerable number of the plates, it was necessary to drive them in pretty firmly with a hammer, otherwise their irregularities would keep them sufficiently separated to allow the heavy weight to fall through a considerable distance and produce a disagreeable shock. After the whole weight had been thus tranferred to the cast-iron columns, the jacks were again forced up against the girders, with a fair pressure, for additional safety. The weight now rested on the iron work.

The old bed was next raised, clear of the masonry, and suspended by rods to the lifting girders. The upper three courses of masonry were then removed. The jacks were then set to one side, and into the clear space, thus made for it, the large wrought-iron bed was moved, correctly placed, and bolted to the caps. The old bed was then lowered onto the new one, the rollers taken out and thrown to the ground, and the old bed-plate temporarily placed on the iron frame which had supported the new bed.

It now remained only to place the steel plates, rollers and saddle beds on the new bed, set the jacks on the ends of the new bed, underneath the transverse girders, lift them high enough to release the shims, which were then removed, and by simultaneously discharging the jacks, the saddles were allowed to settle permanently onto their new beds. This completed the transfer for that tower. Each of the four towers were successively treated in the same manner.

The stone towers had then to be removed. While this was being done it was necessary to temporarily supply the place of the longi-

tudinal bracing, by wooden shores against various parts of the masonry. As rapidly as the sections of iron work were uncovered, the iron struts and rods were put in and secured.

After all the preparations for a transfer were completed, the time consumed in making the change, including the removal of the top three courses of masonry and the old bed-plate, was eight and one-half hours, excepting in the case of the first one which required longer time.

Trains were not allowed on the bridge while the transfer was being made.

The total weight raised by the six jacks was about 650 tons. Two men were at each jack to operate it.

Condition of the Old Towers as Developed by the Progress of the Work.

In cutting away the faces of the old towers at the tops, the stones were found to be cracked through vertically, some of them in several places. Some of the stones were not more than half bedded near the top, so that, in some cases, a two-foot rule could be inserted its whole length and swung around in the space. In such cases the space had to be filled with cement before the cutting was allowed to proceed.

When the weight was lifted from the old bed-plate, it raised in the middle nearly a quarter of an inch. In removing the top three courses, the interior stones were found to be badly shattered in every direction, and in barring them out they flaked into spawls. While the stones were thus broken up, the cement gave indications of having been excellent.

When access to the old rollers was obtained they were found to be embedded in cement and iron rust, the spaces between them and between their ends and the ribs of the bed-plate being filled with this cement, which had become so firm that a chisel bar was required in extricating the rollers. Thus each roller was lying in a trough, and whatever water found its way in there could only escape by evaporation. While this cement had originally allowed the roller some motion, the corrosion caused by the water operating upon the iron surface, kept clean by the heavy rolling pressure, had flattened the bottom sides of the rollers more than $\frac{1}{32}$ inch, and produced depressions in the bed plate to the depth of $\frac{3}{32}$ inch, and in these depressions, assisted by the iron rust and cement, the rollers had come to have a very snug bed. The top of the

roller and under side of the saddle, although slightly rusty, were all right and true.

In view of the condition of the top courses of masonry and of the rollers, there is little reason to doubt that had any successful plan been adopted to resist the swaying of the towers, it must have resulted in their prompt destruction by the consequent grinding down of the top.

On the day when the second transfer was made the thermometer stood at + 7°. On lifting the saddles from the rollers the top of the tower suddenly sprung toward the river ⅜ inch. It can only be conjectured as to how much stress was imposed upon the anchorage in producing this bending of the tower. But it must have been considerable. One circumstance which occurred will aid such conjecture to some extent. The tops of the towers on the Canadian side were found to be about two inches nearer together than their bases, and an effort was made to force them apart with one of the large jacks. The distance was increased ½ inch with little trouble, but when it had increased ⅜ inch the timber struts, against which the jack was pressing, began to buckle and the attempt was abandoned. There was at that time a probable pressure of 60 tons on the jack. When it is remembered that the ⅜ inch is the amount which 60 tons increased the distance by bending the two towers in opposite directions it will be seen that bending one alone ⅜ inch would require a very much greater force.

The removal of the old towers was a somewhat tedious process for the following reasons:

First.—The work was done among the columns, rods, struts, clamps and temporary wooden shores.

Second.—It was done in freezing winter weather.

Third.—Each course was connected with the course below it by several iron dowels.

So far as I could learn no whole stone was found in taking the towers down to the level of the new pedestals, which was as low as they were removed.

In conclusion, I wish to express my satisfaction at the disposition manifested by the gentlemen of the Detroit Bridge and Iron Works to make the workmanship first-class, and also to thank their erecting foreman, Mr. J. H. Stoughton, for his uniform caution in erecting the iron work and for his valuable assistance while making the transfers.

TRANSFERRING CABLES.

NIAGARA RAILWAY SUSPENSION BRIDGE.

L.L.BUCK,
ENGINEER.

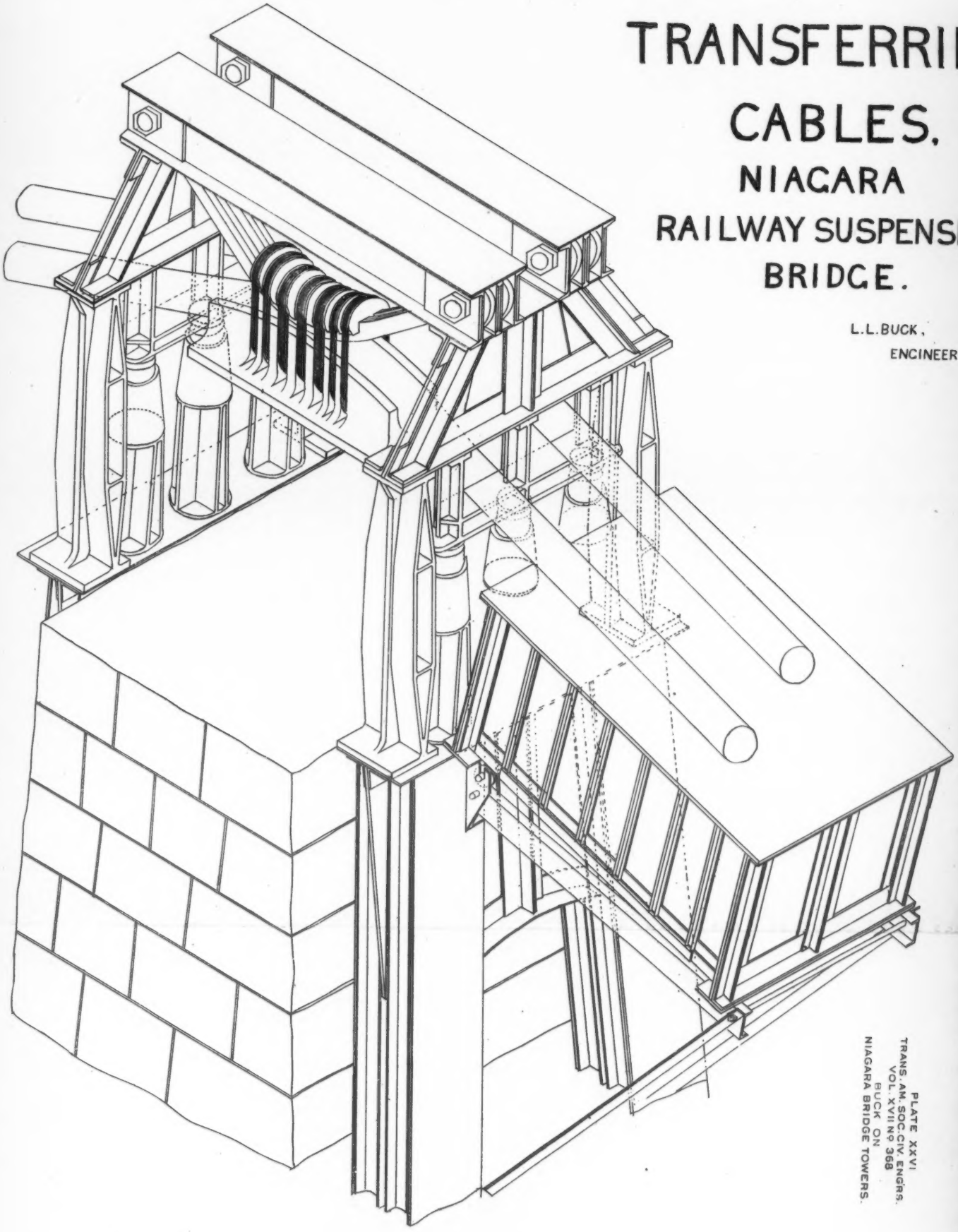


PLATE XXVI
TRANS. AM. SOC. CIV. ENGRS.
VOL. XVII No 368
BUCK ON
NIAGARA BRIDGE TOWERS.

AMERICAN SOCIETY OF CIVIL ENGINEERS.
INSTITUTED 1852.

TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

369.

(Vol. XVII.—November, 1887.)

REPORT OF PROGRESS OF THE COMMITTEE ON THE COMPRESSIVE STRENGTH OF CEMENTS AND THE COMPRESSION OF MORTARS AND SETTLEMENT OF MASONRY.

PRESENTED AT THE ANNUAL CONVENTION, JULY 4TH, 1887.*

Your Committee have been able to make some progress during the past six months, but not so much as they had hoped, and beg leave to make a partial report and continue their work, deferring a final report until such time as they feel they have accomplished that for which they were appointed.

Professor Swain, by the aid of two of his students (Messrs. William C. Cushing and Walter F. Thompson), has extended the experiments upon changes of dimension during induration to our American cements. From an extensive memoir, comprising as well their experiments upon a number of other points foreign to our investigation, we condense the following statement. This was partially reported upon at the annual meeting in January, and we omit the description of apparatus which was then given.†

* Committee: Messrs. F. Collingwood, D. J. Whittemore, W. W. Maclay, T. C. McCollom, George F. Swain, and A. V. Abbott.

† Proceedings, January, 1887, p. 5.

The cements used were F. O. Norton's Rosendale (marked L); Newark and Rosendale (N); James (E); Hoffman (W); Anchor (T); Standard (V); Taylor's Portland (X); and Keystone Portland (Z).

From each mixture of each cement two 5-inch cubes were made, the mixtures being, 1st, neat cement, 2d, one cement, one sand. Of two of the cements, one cement and three sand were also tried.

The points over which measures were taken by the instrument described in our last report were conical-headed brass tacks driven in after a set of from one hour to one day. It was here that the first difficulty arose, as in a few cases irregularities in measurements were traced to imperfect setting of the tacks.

One specimen of each pair of cubes was left in the air, the other in water.

Both horizontal diameters (the measurements are marked A and B respectively in the table) were measured; the gauge reading taken to the ten-thousandth of an inch (this being the unit in the table of differences appended), and the readings were repeated at each observation from three to eight times.

The intervals of the observations were one day, two days; one, two, four, six, eight and twelve weeks. A careful examination of the corrections, due to unavoidable moderate differences of temperature in the measuring apparatus and specimens at the several observations, showed them to be so small as to cause no change in the result.

An examination of the table warrants the conclusions already reached by other observers, that in general:

First.—Cement mortars hardening in air diminish in linear dimensions at least to the end of twelve weeks, and in most cases progressively.

Second.—Cement mortars hardening in water increase in like manner but to a less degree.

The experimenters claim that the exceptions to these rules seen in the table are due to inaccurate measurements, the first measurements being in some cases taken too soon, when the tacks were not rigid. Other causes were mentioned which it is not necessary to detail. The changes for the first two weeks are not so regular as for subsequent periods.

Third.—The contractions and expansions are greatest in neat cement mortars.

Vol. XVII, page 214.

TABLE No. 1.

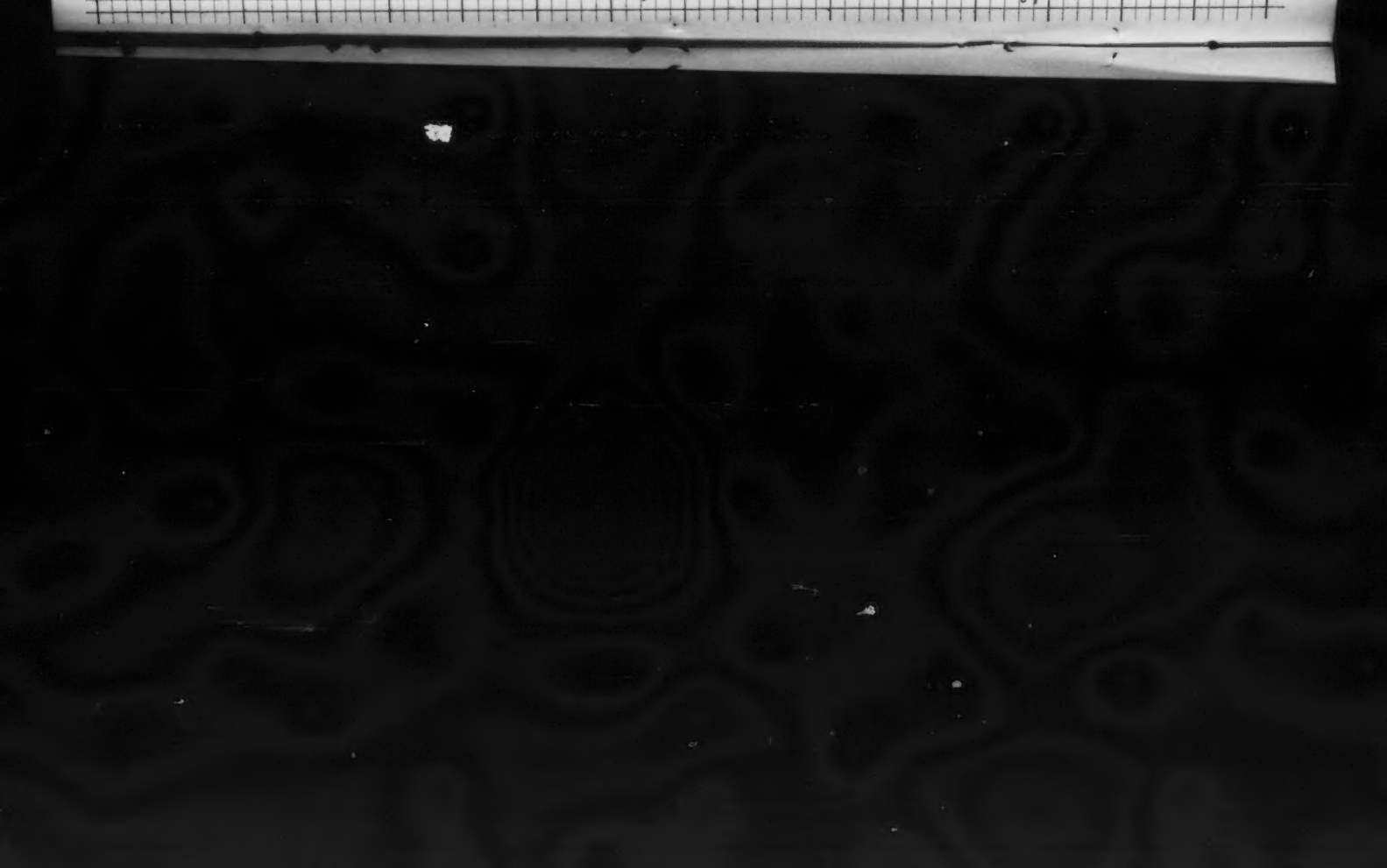
Increase or decrease in sides of 5-inch cubes from the first day after mixing up to the age of twelve weeks. The units expressing the differences are each $\frac{1}{10000}$ inch.

\+ denotes increase, — denotes decrease.

The first reading in each case is for the neat cement; the second for 1 cement to 1 sand; the third for 1 cement to 3 sand.

Designation of cement.	Volume cement.	Volume sand.	Per cent. of water.	1 day in Water. A	1 day in Water. B	1 day in Air. A	1 day in Air. B	2 days in Water. A	2 days in Water. B	2 days in Air. A	2 days in Air. B	4 days in Water. A	4 days in Water. B	4 days in Air. A	4 days in Air. B	1 week in Water. A	1 week in Water. B	1 week in Air. A	1 week in Air. B	2 weeks in Water. A	2 weeks in Water. B	2 weeks in Air. A	2 weeks in Air. B	3 weeks in Water. A	3 weeks in Water. B	3 weeks in Air. A	3 weeks in Air. B	4 weeks in Water. A	4 weeks in Water. B	4 weeks in Air. A	4 weeks in Air. B	6 weeks in Water. A	6 weeks in Water. B	6 weeks in Air. A	6 weeks in Air. B	8 weeks in Water. A	8 weeks in Water. B	8 weeks in Air. A	8 weeks in Air. B	12 weeks in Water. A	12 weeks in Water. B	12 weeks in Air. A	12 weeks in Air. B
E	1	0	30	+ 4	−15	+ 7	+12	−13	+ 7	− 2	− 1	+ 1	−10	....	....	+ 8	+ 5	− 4	− 5	+11	+ 3	−14	0	+12	+ 7	−34	−38	+26	+15	−49	−50	+39	+26	− 70	− 65	+40	+33	− 88	− 86	+ 49	+ 38	−117	−119
E	1	1	15	+ 0	+ 0	+ 2	0	....	....	− 1	− 4	+ 2	+15	....	....	+13	− 6	+ 1	− 4	+ 6	−12	−10	− 9	+ 5	+ 1	−13	−12	+ 6	− 1	−21	−19	+ 9	− 6	− 34	− 32	−14	− 4	− 35	− 32	+ 12	− 2	− 54	− 48
E*	1	3	12	+32	+ 5	− 4	− 9	−22	− 2	− 9	− 9	....	....	....	....	−22	− 7	− 9	− 9	−10	− 5	−13	−17	−40	− 3	−18	−19	−30	+14	−17	−19	−40	+17	− 16	− 27	−47	+15	− 17	− 26	− 64	+ 11	− 24	− 34
T	1	0	30	0	− 5	−19	− 9	− 5	− 7	−10	− 1	....	....	....	....	− 2	− 1	−17	−15	+ 3	+ 2	−20	−15	....	....	....	....	+16	+14	− 47	−39	+34	+34	− 71	− 58	+39	+44	− 83	− 69	+ 47	+ 47	−107	−108
T	1	1	15	+ 4	+ 1	− 4	+ 2	+ 7	− 2	....	....	....	....	....	....	+13	+ 6	+ 1	+ 5	+ 9	+ 3	− 7	+ 1	....	....	....	....	+20	+12	− 18	−10	+26	+19	− 27	− 20	+30	+15	− 32	− 26	+ 32	+ 15	− 49	− 41
T	1	3	12	....	....	+23	+33	....	....	+21	+41	....	....	....	....	....	....	+26	+39	....	....	+21	+34	....	....	....	....	....	....	+16	+18	....	....	+ 16	+ 21	....	....	+ 6	+ 12	....	....	+ 3	0
W	1	0	30	− 6	0	− 8	+ 1	− 6	+ 5	− 4	+ 3	....	....	....	....	+11	+15	+ 9	+ 9	+19	+23	+ 3	+ 3	....	....	....	....	+21	+25	− 17	− 22	+23	+24	− 34	− 33	+23	+23	− 53	− 59	+ 21	+ 20	− 84	− 93
W	1	1	15	....	..	0	− 5	+ 5	− 1	− 1	0	....	....	....	....	+ 2	+ 6	− 1	− 1	+12	+17	−10	− 5	....	....	....	....	+ 6	+ 9	− 15	−10	+13	+10	− 33	− 38	+ 1	+ 4	− 41	− 41	− 1	+ 7	− 51	− 55
V	1	0	30	+ 6	+ 6	+ 4	+12	+17	+10	+ 3	+ 7	....	....	....	....	+12	+17	+ 7	+12	+12	+15	− 8	− 1	....	....	....	....	+26	+ 21	− 44	− 33	+41	+34	− 70	− 63	+50	+44	− 85	− 69	+ 57	+ 54	−116	−111
V	1	1	15	− 8	− 2	+ 2	− 8	− 4	− 4	− 7	− 5	....	....	....	....	0	− 2	− 4	+ 2	−17	−14	−13	−15	....	....	....	....	−12	− 4	− 27	− 28	− 8	− 2	− 37	− 38	− 4	+ 3	− 46	− 45	+ 12	+ 13	− 50	− 48
N	1	0	27	+ 3	+ 8	− 9	+ 1	+ 6	+ 2	−17	− 9	....	....	....	....	+16	+21	−22	−19	+27	+25	−37	−33	....	....	....	....	+48	+48	− 79	− 79	+69	+69	−114	−106	+88	+92	−139	−118	+121	+123	−145	−142
N	1	1	14	− 2	+ 3	− 2	−13	+ 8	0	− 1	−12	....	....	....	....	−11	−13	−14	−35	− 3	− 3	−20	−36	....	....	....	....	+10	+ 5	− 36	− 60	+18	+18	− 41	− 75	+20	+20	− 43	− 81	+ 37	+ 41	− 72	− 85
X	1	0	27 to 28	− 2	− 4	+ 1	− 3	− 1	− 1	0	+ 2	....	....	....	....	−10	− 7	− 9	−10	− 1	−49	−12	−15	....	....	....	....	+ 5	+ 5	− 34	− 38	+16	+18	− 48	− 53	+16	+18	− 54	− 69	+ 33	+ 39	− 71	− 75
X	1	1	15	− 8	− 9	− 3	− 4	− 4	− 5	− 7	−15	....	....	....	....	−18	− 6	− 7	−12	−10	− 9	− 7	−15	....	....	....	....	− 9	−13	− 16	− 27	− 9	− 2	− 33	− 42	− 2	0	− 36	− 43	+ 4	+ 5	− 41	− 41
Z	1	0	18	+ 2	+ 2	− 3	− 2	− 5	+14	+ 5	+ 3	....	....	....	....	+ 7	+20	−14	−12	+10	+17	−38	−36	....	....	....	....	+23	+22	− 71	− 44	+18	+26	− 90	− 63	+21	+31	−100	− 71	+ 19	+ 30	−111	−103
L	1	0	20	+17	+19	− 3	0	+19	+27	− 2	0	....	....	....	....	+38	+41	− 3	0	+44	+47	−23	−14	....	....	....	....	+47	+50	− 61	− 53	+58	+59	− 98	− 93	+68	+65	−128	−117	+ 69	+ 70	−158	−159

* Tack in face A was incorrect.



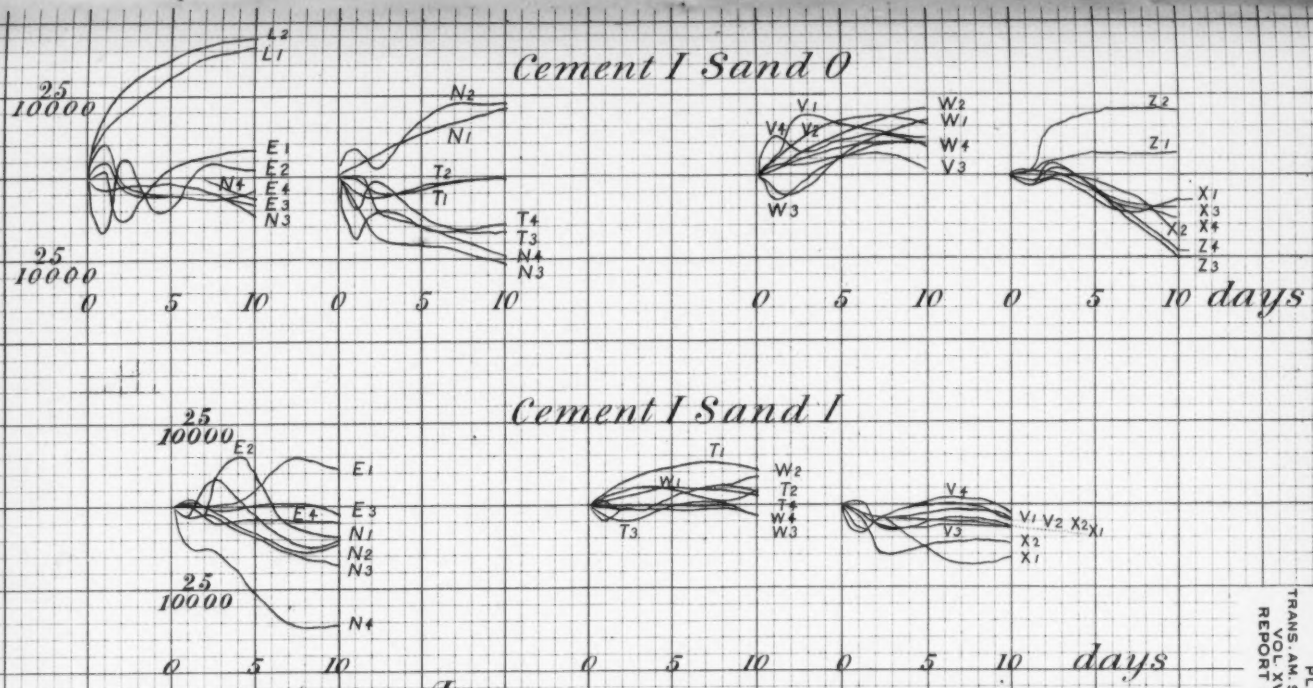


PLATE XXVII
TRANS. AM. SOC. CIV. ENGRS.
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REPORT ON CEMENTS.

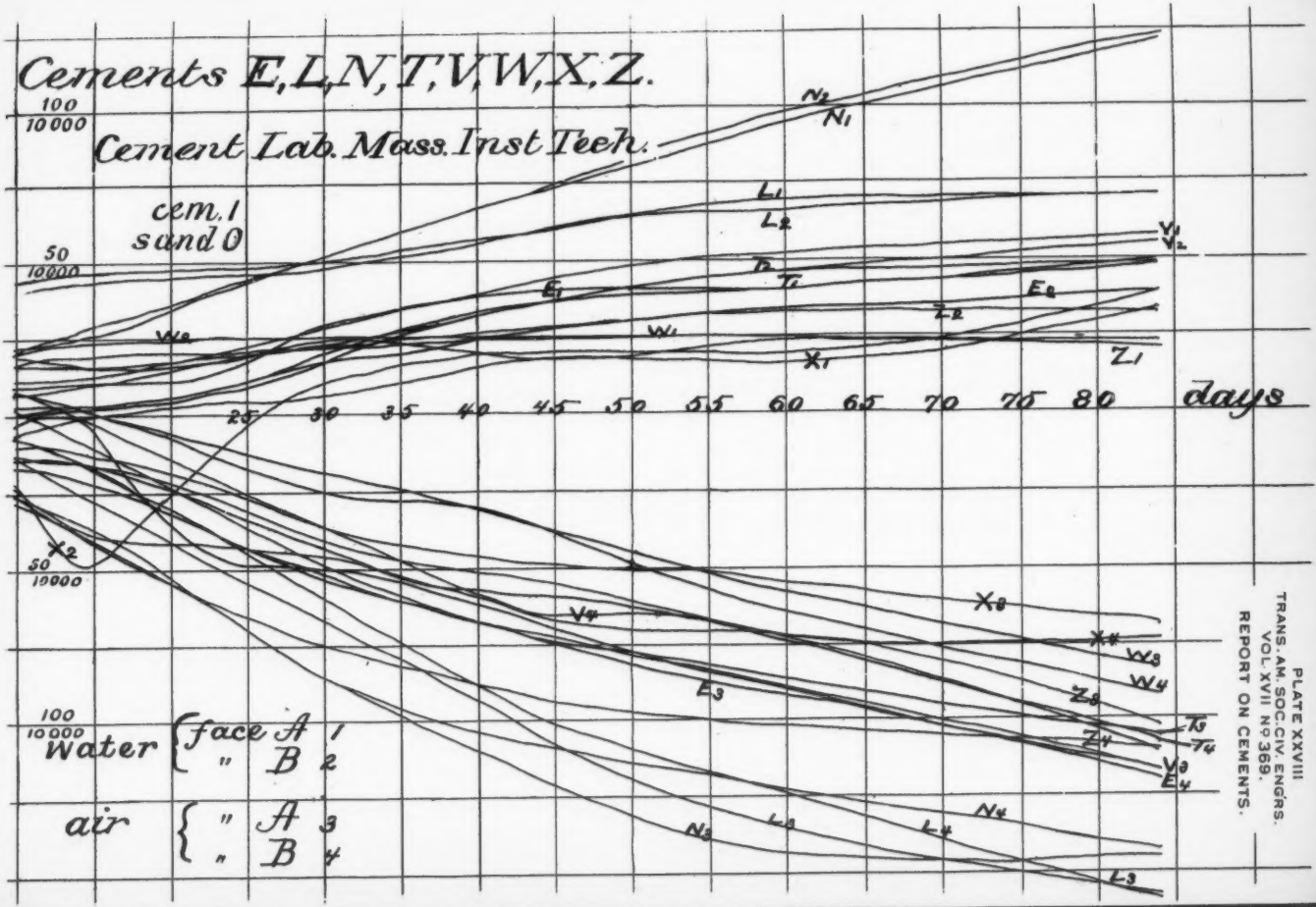

PLATE XXVIII.
TRANS. AM. SOC. CIV. ENGRS.
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REPORT ON CEMENTS.

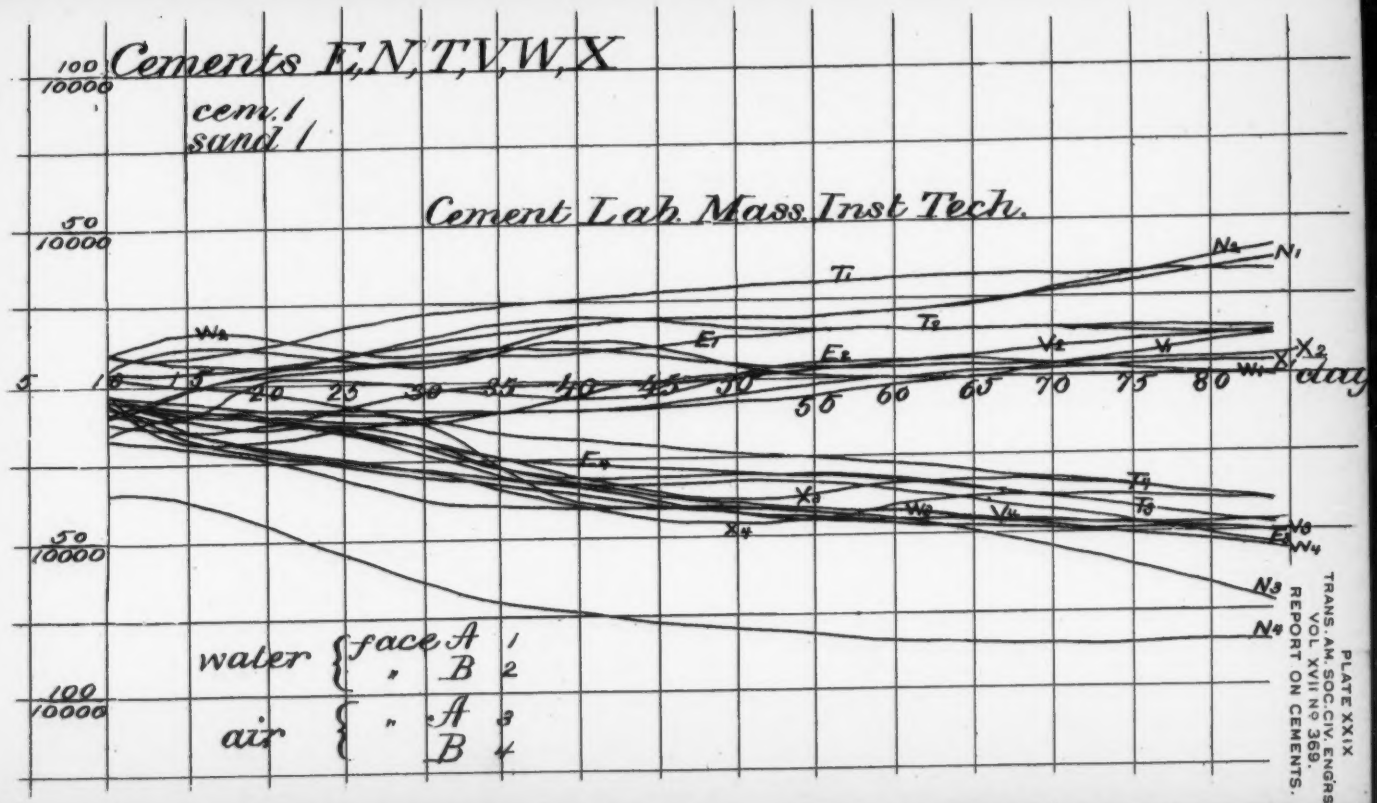

PLATE XXIX
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REPORT ON CEMENTS.

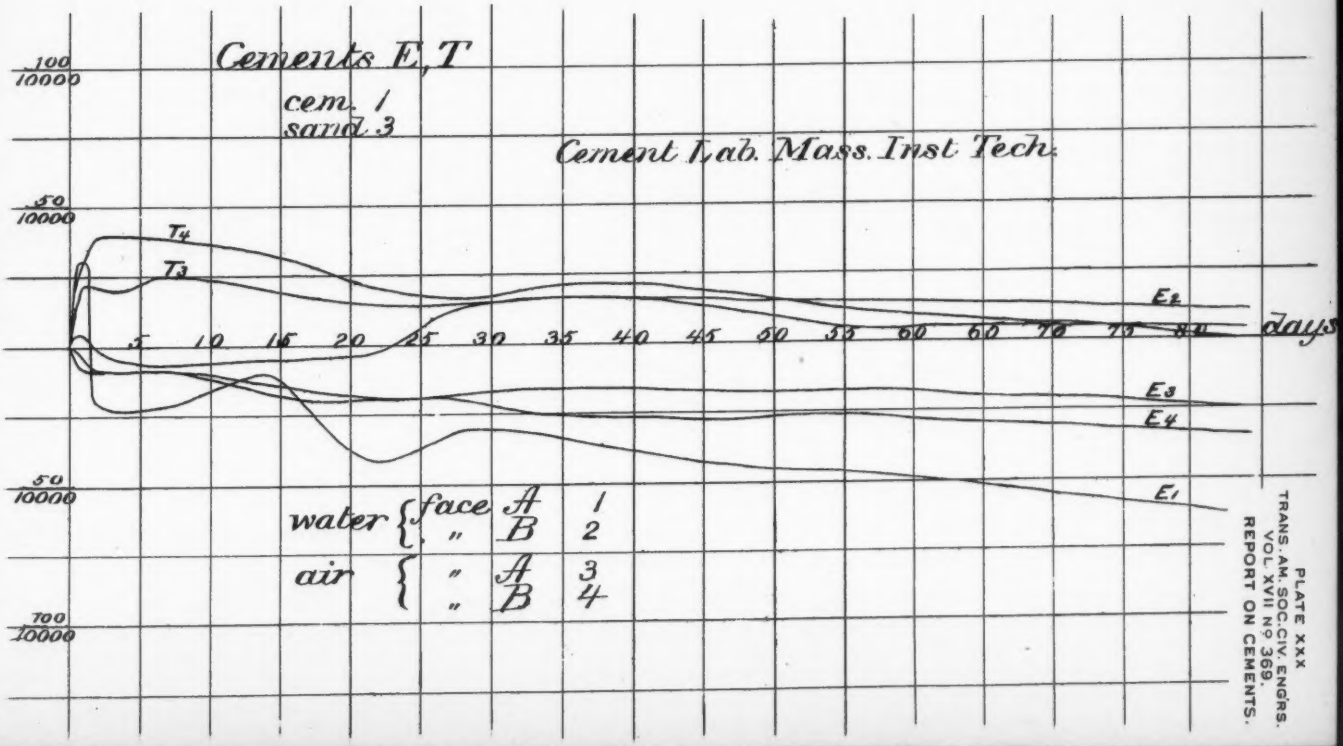

PLATE XXX
TRANS. AM. SOC. CIV. ENGRS.
VOL XVII Nº 369.
REPORT ON CEMENTS.

PLATE XXXI
TRANS. AM. SOC. CIV. ENG'RS.
VOL XVII Nº 369.
REPORT ON CEMENTS.

Fourth.—The quick setting cements N and L show the greatest contraction when neat. W, which is also quick setting, does not however follow the rule.

The expansion of the quick setting cements is also greater.

Fifth.—The changes are less in mortars containing sand.

Sixth.—The changes are less in water than in air.

Seventh.—The contraction is at the end of twelve weeks:

For neat cement.....0.14 per cent. to 0.32 per cent.
" one cement, one sand ..0.08 " 0.17 "

Eighth.—The expansion at the end of twelve weeks is:

For neat cement.....0.04 per cent. to 0.25 per cent.
" one cement, one sand ..0.0 " 0.08 "

Ninth.—The contraction or expansion is essentially the same in all directions.

By comparing these figures for American cements with those given by Bauschinger for foreign brands, there is a strking agreement.

He gives (see our previous reports) for neat cement, contractions in air in sixteen weeks of 12 to 34 per cent., and for one cement to three sand, 8 to 15 per cent. For expansion of neat cement in water in sixteen weeks 1 to 15 per cent., and for one cement to three sand, 0 to 2 per cent. The expansions being less in his experiments. The experimenters on our American cements however, consider the 25 per cent. expansion (Specimen N in the table) as probably erroneous, for reason given by them.

Comparing the average co-efficients of expansion, we have:

For nine cements, neat, by Prof. Bauschinger.. 0.00048 in 16 weeks.
" English Portland, neat, by Mr. Grant..... 0.00084 " 3 months.
" American cements, neat, by present table... 0.00105 " 12 weeks.
" " " omitting the one claimed in error..... 0.00085 " 12 "

agreeing closely with Mr. Grant's.

Plates XXVII to XXX have been prepared from the table to illustrate the subject graphically, and the Plate XXXI shows the same for Professor Bauschinger's table.

Mr. Maclay, of the committee, has sent a few results of experiments on absorption which are of interest. From accurate weighings he gives the following table of gains in weight in perfectly dry mortars after immersion in fresh water for the times stated.

Cement.	Sand.	12 hours' immersion.	36 hours' immersion.	7 days' immersion.	79 days' immersion.
Rosendale.....1	2 parts.	11 per cent.	11 per cent.	11 per cent.	15.40 per cent.
" 1	1 part.	11 "	11 "	11 "	15.06 "
Portland 1	3 parts.	9.2 "	10.9 "	10.9 "	15.08 "
" neat.....1	0 part.	11.2 "	11.2 "	12.06 "	13 08 "

The mortars, it is seen, gained less than the neat Portland up to seven days, after which the neat Portland absorbs least.

Mr. Maclay also sends some results of compression tests on cylinders 24½ inches long and 4⅛ inches diameter.

TABLE No. 2.

Compression tests of three cylinders 24½ inches long and 4⅛ inches in diameter (area 13 $\frac{3}{10}\frac{8}{0}$ square inches) made from (Hammil & Gillispie's) Portland cement.

Neat Cement, 104 days old.			1 Cement, 2 Sand, 3 mos. old.		1 Cement, 3 Sand, 2 mos. old.	
Load lbs. sq. in.	Comp. inches.	Differences.	Compression inches.	Differences.	Compression inches.	Differences.
100	0.0237				0.0062	
150	.0250	13	0.0010		.0095	33
200	.0267	17	.0023	13	.0135	40
250	.0283	16	.0035	12	.0175	40
300	.0300	17	.0050	15	.0220	45
350	.0323	23	.0073	23	.0253	33
400	.0352	29	.0097	24	.0290	37
450	.0375	23	.0115	18	.0330	40
500	.0402	27	.0142	27	.0383	53
550	.0427	25	.0168	26	.0433	50
600	.0450	23	.0190	22	.0480	47
650	.0473	23	.0210	20	.0567	87
700	.0495	22	.0230	20	.0653	86
750	.0515	20	.0270	40	750 pounds crushed.	
800	.0535	20	.0290	20		
850	.0553	18	.0310	20		
900	.0570	17	.0330	20		
950	.0587	17	.0367	37		
1 000	.0600	13	.0403	36		
1 050	.0610	10	.0450	47		
1 100	.0630	20	1 125 pounds crushed.			
1 150	.0643	13				
1 200	.0650	7				
1 250	.0677	27				
1 300	.0697	20				
1 350	.0710	13				
1 400	.0737	27				
1 450	.0753	16				
1 500	.0760	7				
1 550	.0770	10				
1 600	.0777	7				
1 650	.0780	3				
1 700	.0793	13				
1 750	.0813	20				
1 800	.0840	27				
1 850	.0847	7				
4 358	Crushed.					

The first test in the table is of neat cement at a little over three months old. The results, it will be seen, are somewhat irregular, and no one point can be assigned as a limit of elasticity.

On the contrary, the compressions for equal increment of strain grow gradually less.

At 200 pounds per square inch the co-efficient of compression per 100 pounds of load is 0.00054; at 500 pounds it is 0.00033; at 700 pounds it is 0.00029; at 1 000 pounds, 0.00024; at 1 500 pounds, 0.00021; and at 1 850 pounds, or just before crushing, it is 0.00019.

Examining the similar prism mixed of one cement and two sand, we find co-efficient at 200 pounds is 0.000047; at 500 pounds, 0.000116; at 700 pounds, 0.000138; at 1 000 pounds, 0.000164; the specimen crushing at 1 125 pounds.

Here we see the change is in the opposite direction, gradually increasing; beginning with less than one-tenth of that of neat cement, and at 1 000 pounds being two-thirds of that of neat cement.

Examining the prism mixed three to one, and two months old, we get at 200 pounds, 0.000276; at 500 pounds, .000313; at 700 pounds, 0.000381; being one-half that of the neat cement at 200 pounds, and one-third greater at 700 pounds.